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MIND
A QUARTERLY REVIEW
OF
PSYCHOLOGY AND PHILOSOPHY.

EDITED BY

PROF. G. F. STOUT,

WITH THE CO-OPERATION OF PROFESSOR E. B. TITCHENER, AMERICAN EDITORIAL REPRESENTATIVE, AND OF PROFESSOR WARD, PROFESSOR PRINGLE-PATTISON, DAVID MORRISON, M.A., AND OTHER MEMBERS OF AN ADVISORY COMMITTEE.

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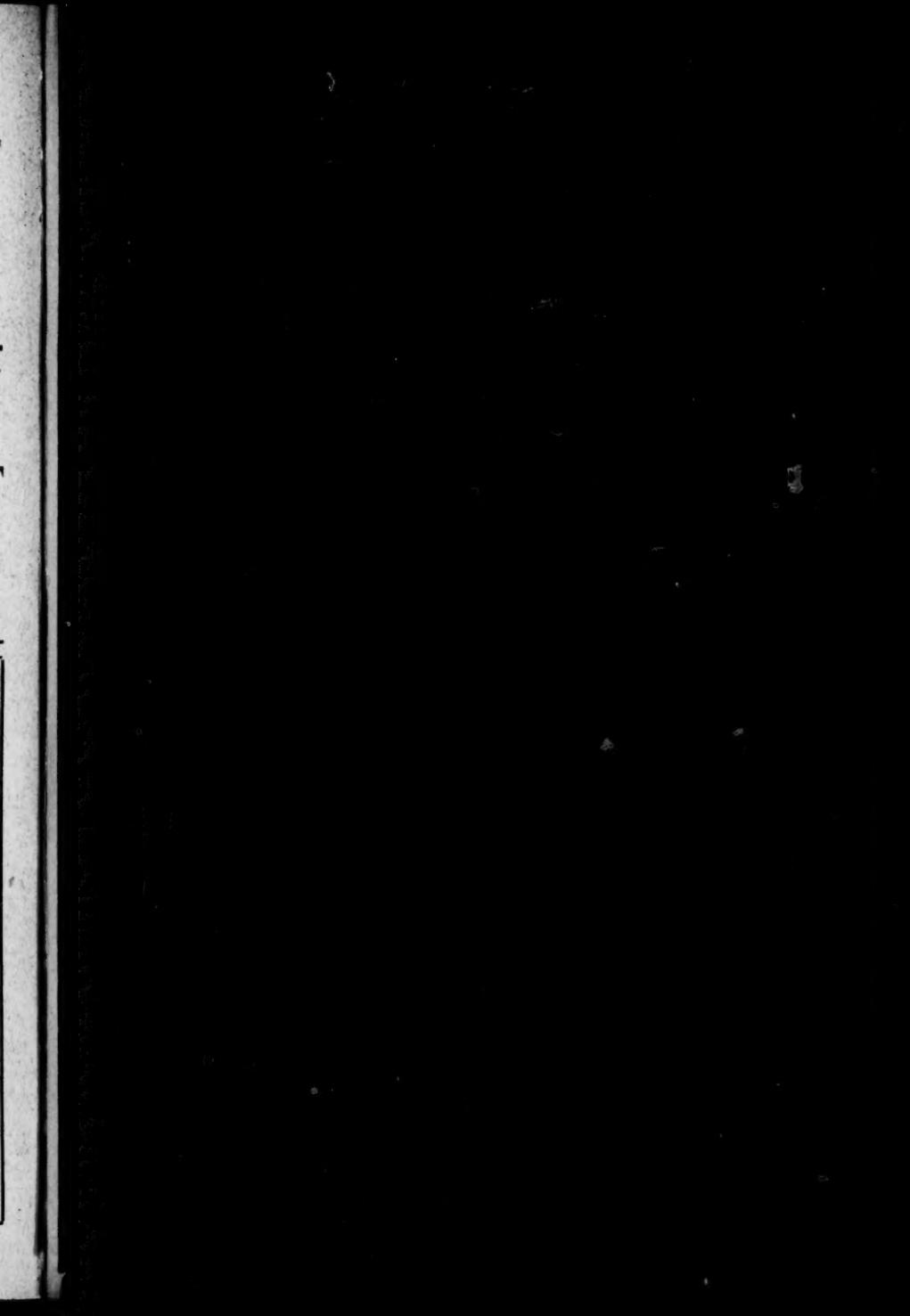
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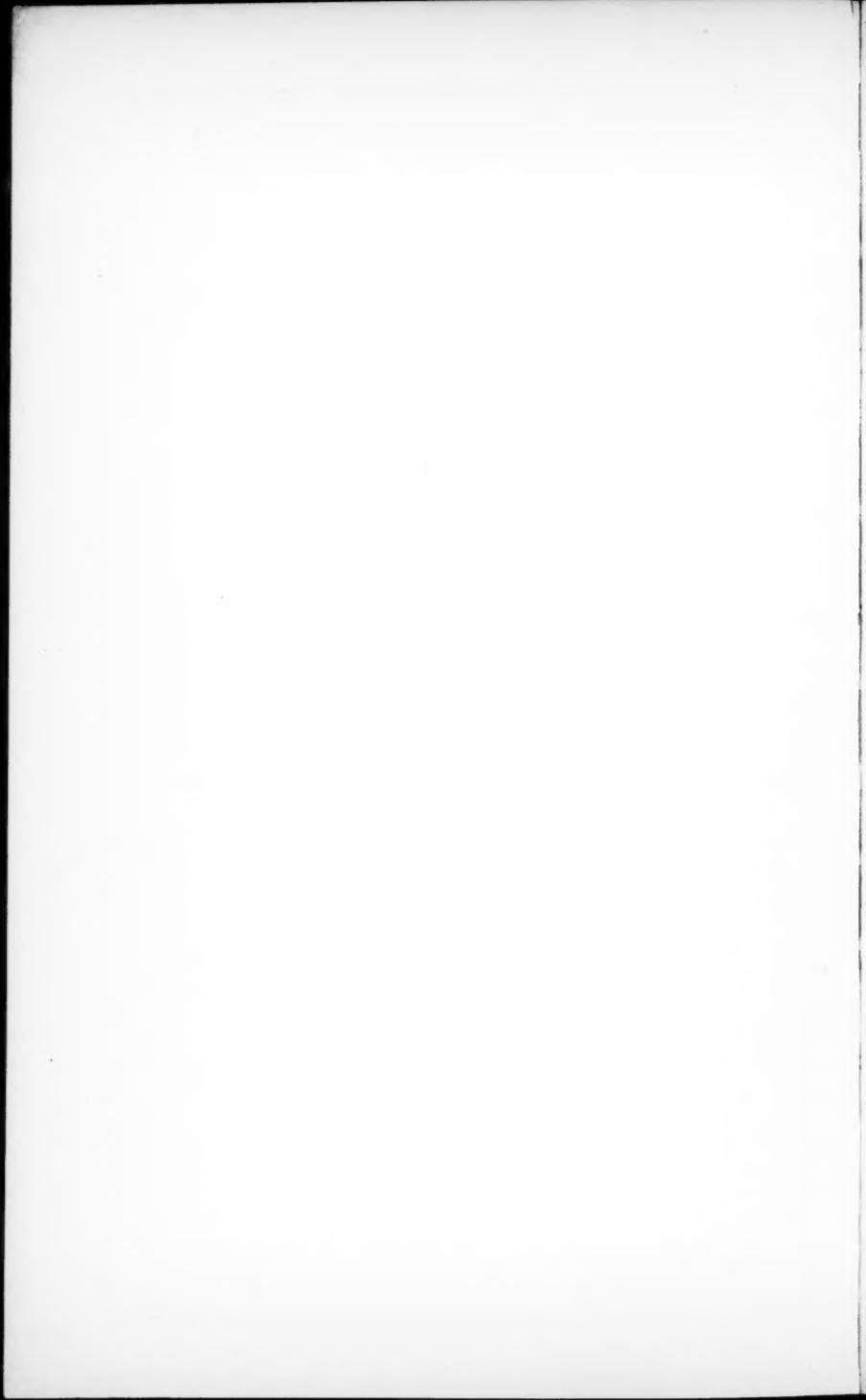
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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY



I.—ENJOYMENT AND AWARENESS.

BY C. LLOYD MORGAN.

IN an article in *Scientia* (vol. xviii., 1915) on "Mind and Body" I contended that psychical process always implies correlated physiological process, as this in turn implies physical process. To avoid cumbrous repetition of such phrases, I termed physical process *a*-process, physiological process with its physical implicate *ab*-process, and psychical process, with its physiological and physical implicates, *abc*-process. I urged that a criterion of the presence of *abc*-process, in any organism, is not only awareness but prospective awareness, or what I termed pre-awareness. And, giving rein to speculation, I ventured to suggest that a relation of awareness might be regarded as ubiquitous at all stages of natural process.

Having been told that the suggested ubiquity of awareness was left quite vague, and that what I meant by awareness is very indefinite, I submit the following notes which may serve to throw some further light on what I had in mind:—

1. Let *x* be an inorganic system abstractedly regarded in isolation, and let it be a theatre of physical change (*a*-process). Assume as a basis for speculative discussion that what may thus be known by us as physical process is also a primitive mode of enjoyment.¹

2. Let *y* be a second system analogous to *x*, compresent with it in space, and in effective physical relatedness to it.

¹ I here borrow Prof. Alexander's convenient term, parting company with him, I fear, in the treatment which follows.

Then in virtue of this physical relatedness the *a*-process in *x* (or in *y*) is changed in presence of *y* (or of *x*).

3. The *a*-process in each being thus changed, the enjoyment in each is, *ex hypothesi*, also changed; for it is part of our initial assumption that with every difference in *a*-process there is a correlated difference in enjoyment. The change of enjoyment in *x* or in *y* is thus also correlated with physical changes in *y* or in *x*. This relation of enjoyment to a physical object compresent in space is a mode of what we will call awareness. The terms in relation are *x* and *y*. We may say that *x* in which a change of enjoyment occurs is aware of *y*; and similarly *y* of *x* (*cf.* Note 26).

4. On this view the enjoyment is primarily immanent in any given system; but awareness is a transeunt influence from some other system, external to it, but compresent with it in space. Hence awareness of *y* implies enjoyment in *x*.

5. But though compresent in space the changes in *y* are not necessarily coincident in time with changes in *x*. The physical process in *y* may precede the change of enjoyment in *x*; but the change of enjoyment in *x* cannot precede the physical process in *y*. As I read the problem of time, there can be no immediate awareness of an event which has not yet occurred (*cf.* Notes 32 ff. on pre-awareness).

6. If, now, *x* be a system composed of molecules each a theatre of *a*-process, then there is enjoyment within each molecule regarded in isolation; and as a constituent of the systemic whole, each is aware of the others as compresent, in so far as its *a*-process and its enjoyment are influenced by these others.

7. If, on the other hand, *x* be a constituent part of a much more complexly integrated system, then the integrated totality of enjoyment in that system will be correlative with the systemic totality of *a*-process in the complex whole; and these totalities may be reached not only by additive summation or quantitative variation, giving *resultants*, but by constitutive evolution giving *emergents* (see G. H. Lewes, *Problems of Life and Mind*, Prob. v., chap. iii.; *cf.* J. S. Mill, *System of Logic*, Bk. III., chap. vi., "heteropathic laws").

8. Thus the type of molecular process in a vapour differs from that in a liquid, and this again from that in a solid. These differences are not only quantitative; they depend also on constitutive relations; so that there are distinctive properties of vapour, liquid, and solid, explicable in terms of molecular theory. So too the properties of water differ from those of its elementary factors, not yet or no longer thus combined. Here, as in the case of all chemical com-

ENJOYMENT AND AWARENESS.

pounds, the difference in emergent properties is the index of a difference in constitutive relations within the inorganic system.

9. If then, in accordance with our initial assumption, any given physical process, as we know it, is also a mode of enjoyment, it may be urged that there are qualitatively different modes of enjoyment in vapour, in liquid, and in solid ; and that there is a specific water-enjoyment, as contrasted with an oxygen- or a hydrogen-enjoyment.

10. And if the physical process in one system is modified by transeunt causation proceeding from other compresent systems, the enjoyment in the one is also modified through its relation of awareness to the others. This modification may be merely additive, with quantitative change in extent or intensity ; or it may also be constitutive with emergent qualities of enjoyment.

11. The emergence of new properties in that of which there is awareness, *i.e.* in physical systems compresent in space ; and the emergence of new qualities in that which is aware of them, *i.e.* in modes of enjoyment within a given system, provide for progressive evolution of what we will call *qualia*, as a general term for emergent or constitutive characters.

12. The point of emphasis here is that there are emergent characters in enjoyment, and that the evolution of *qualia* is not restricted to the objective term in the awareness-relation. This seems to follow from our initial assumption ; for since in the evolution of physical systems there seem to be emergent *qualia*, and since *ex hypothesi* there is in each system its own enjoyment, it appears to be legitimate to regard this enjoyment as susceptible of constitutive evolution.

13. The question here arises whether we should regard the relation of awareness as itself susceptible of differentiation. This does not seem to be necessary. The modes of effective physical relatedness of one inorganic system to another may have assignable differences ; physical systems may be in different "fields of force," so-called. But we may provisionally assume that, while the objects of awareness have their distinctive *qualia*, and while the enjoyment may be specifically qualified, the transeunt awareness is one and the same in kind irrespective of the qualification of its terms.

14. Pass now to the case in which the complex system is an organism. Here both the process which the biologist seeks to interpret and the enjoyment, the presence of which we assume, have the *qualia* which are characteristic of the living. So distinctive are these *qualia*, in their objective

aspects, that there is, as I believe, justification for saying that what I have called *ab*-process supervenes upon *a*-process. By an *ab*-process I mean one that is not only physical but has also physiological *qualia*. And I suggest that, just as *ab*-process supervenes upon *a*-process, so does a new mode of enjoyment therein, with its emergent *qualia*, supervene upon its inorganic predecessors in the course of constitutive evolution.

15. When the *x* and *y* with which we started are approximately similar terms the enjoyment in *x* as modified by awareness of *y*, is approximately similar to the enjoyment in *y* as modified by awareness of *x*. But in the course of evolution one term, *m'*, may be a centre of differentiation and integration much more complex than those which obtain in other terms, *n'*, *q'*, *r'*, etc. The enjoyment in *m'* may be much more complex and possess specific *qualia*, while the enjoyments in *n'*, *q'*, *r'* may have undergone far less development. It does not follow that because *m'*, with its highly evolved enjoyment, has that enjoyment specifically modified by awareness of *n'*, *q'*, *r'*, any of these, with its lower modes of enjoyment, has that enjoyment modified in like manner by awareness of *m'*.

16. In other words the enjoyment in an organism, say a plant, may be changed by awareness of inorganic bodies compresent in space; but the enjoyment in these bodies may be changed in far less degree, and so to speak on a much lower plane, by awareness of the organism which is compresent with them.

17. Can we say in what respect, irrespective of emergent *qualia*, *ab*-process, as specific to the organism, differs from *a*-process? Apart from that on which scientific vitalism lays stress, namely the close-knit integration of differentiated sub-processes into one nicely-balanced process which is the life of the organism as a whole, we may here also lay stress upon this; that it differs in the marked emphasis on "prospective value". For the organism as such, however, there is only immediate awareness of that, external to the system, which directly affects the enjoyment in that system by way of transeunt awareness. There is as yet no representative pre-awareness (see Notes 32 ff.).

18. None the less such immediate awareness may have prospective value. Since *ab*-process in the organism as a whole is characterised by a peculiar type of cyclic routine, certain modes of awareness at stage *e'* of that routine may have prospective value in so modifying the course of routine as to meet its requirements at a later stage *g'* or *k'*. The

transeunt process, with awareness, at e' , may prepare the organism and its enjoyment for what will happen at g' or k' , of which it is at e' wholly unaware, since, as I hold, there is no transeunt influence from that which is not yet in being.

19. The fertilisation of the ovum has prospective value in terms of the subsequent development of the embryo. In that embryo the optic vesicle, invaginated by ectoderm, has prospective value in that therefrom the eye will be developed. And this embryonic eye has prospective value for the direct awareness in vision which will follow in due course. But there is no prospective awareness in the optic vesicle of anything of the nature of a future object of vision. Still the enjoyment is being progressively developed so as to render it susceptible of such later relation of awareness.

20. Of course within the organism each several part and organ has, *ex hypothesi*, awareness of the functioning of other parts and organs; and more immediately and directly of the composition of the blood with its biochemical constitution as modified by the presence of organic secretions or hormones, and so forth—such awareness carrying no implication of “knowledge about”. And this intra-organic awareness is intimately connected with the unity of the total life-process as it is interpreted by the scientific school of vitalists, and as it is organically enjoyed.

21. When, in one of the higher animals, a central nervous system is developed, we seem justified in regarding that system as an *imperium in imperio*; we seem justified in regarding it as a theatre of privileged enjoyment, and speaking of it as playing the part of m' (Note 15) to other organs as n' , q' , r' , etc. At a yet later stage of development the cortex is differentiated, and we have an *imperium in imperio in imperio*. The cortex is the seat of a yet more highly privileged enjoyment with emergent *qualia*. It is aware of processes in the lower nerve-centres and, through their intervention, of processes external to them *via extero-, entero-, and proprio-*ceptors.

22. This more highly privileged enjoyment is probably the dominant factor in that of the infant in the first few months, and, presumably, in that of a great number of animals. With the advent of conceptual thinking, with its underlying *ab-*process, the totality of enjoyment is raised to a yet higher level, is rendered more unified and systematic, and gains its highest known *qualia*.

23. Provisionally we may say that such privileged cortical enjoyment has the *qualia* of consciousness; and that changes in this enjoyment through awareness are of the same kind

as those of which we have experience. As an example of a distinctive *quale* of conscious enjoyment, as modified by awareness, we may take our awareness of colour in objects of vision ; as an example of yet higher *qualia*, we may take any one of the so-called tertiary qualities, which are correlated with certain emergent characters of human enjoyment.

24. Follow up a little further our awareness of colour. Let a spectrum on a screen be the compresent physical system. As interpreted by physics there proceeds from this spectrum an ordered series of electro-magnetic undulations differing in wave-length. The variation in this respect from one end of the spectrum to the other, and beyond its visible limits, appears to be quantitative and quantitative only. There seems to be no hint of any difference of emergent properties from the physical standpoint.

25. But let that which is related to the spectrum by way of awareness be an organism in which a retino-cerebral system has been developed, then the total enjoyment in that organism is so modified by the compresent spectrum as to have emergent qualitative differences in colour-enjoyment and not merely quantitative differences. As at present advised therefore I am led to conclude that colours are qualities of enjoyment emergent within the organism and not emergent properties of the compresent object to which that enjoyment is related by awareness.

26. The difficulty here arises that what I speak of as the quality of enjoyment is referred to the compresent object. This difficulty is really that involved in the whole problem of reference. On this head it must suffice to suggest that while the awareness with which I have dealt is a transeunt influence proceeding inwards from the object of awareness to enjoyment, reference retraces the course outwards from enjoyment to object ; and that all that is gained in enjoyment *via* awareness throughout the whole of its inward course, is projected outwards in reference to the object. And since both our action and our language conform to this reference, we do not often pause to consider whence this reference comes—namely from enjoyment within the organism.

27. It may, however, be urged that awareness always implies reference. I am not concerned to deny the implication. It is not improbable that even the most primitive cases of awareness (as in Note 3) imply some primæval form of reference to that of which there is awareness. In this sense some germinal form of reference may be complementary to any germinal phase of awareness. And al-

though I have dealt with awareness as a transeunt influence from without by which enjoyment is modified, I freely acknowledge that in psychological development the implied reference receives the greater emphasis—so much so that awareness comes to mean reference, and the distinction I draw may appear somewhat strained.

28. What I am chiefly concerned to urge is that, in any case, the object of reference is characterised by the *qualia* of enjoyment evoked by awareness of a compresent system. These *qualia* are, so to speak, reflected from enjoyment on to the correlative object. By object I here mean that from which awareness comes, but solely in respect of this immediate and direct awareness. That is to say I abstract from all that is meditately or indirectly suggested. When there is direct reference of colour to that from which awareness proceeds in sensory presentation, there are, I suggest, projected *qualia* of enjoyment which may be correlated with physical vibrations and so forth.

29. In some cases, however, the projected *qualia* of enjoyment, though they are referred to the object of vision, have no correspondent correlates in the compresent system, interpreted as a physical object. If, on a purple disc, alternate sectors of black and white be so arranged as to give, on rotation of the disc, a ring which, from the physical point of view, has no preponderance of those vibrations which are normally correlated with colour, that ring appears to be green. This tinting is, as we say, retino-cerebral in origin. None the less it is referred to the ring on the disc, and we say that it is this part of the disc itself, as an object of vision, which is tinted. Such contrast effects in vision seem to arise in retino-cerebral enjoyment; but they are referred to the object with which we are compresent in space.

30. If colour as a secondary quality be thus regarded as a projected *quale* of perceptual enjoyment, so must the tertiary qualities—esthetic and ethical values and the like—be regarded as projected *qualia* of our conceptual enjoyment. At their highest they emanate from that crowning phase in the development of human enjoyment to which the term spiritual is commonly applied (see Note 49).

31. Thus there is an ascending hierarchy in the *qualia* of enjoyment. At the lowest or inorganic level the *qualia* of enjoyment, as referred to that from which awareness proceeds, may differ but little from the physical properties of compresent objects. At the highest or spiritual level they transform the world on which they are projected and determine our conduct therein. But only, as I conceive, through

the systematic linkage of the whole ascending series of enjoyments, from bottom to top, can all the modes of our supreme and highly integrated enjoyment be interpreted. Our enjoyment, with all its *qualia*, is the net result of the constitutive totality of enjoyments, including those at all levels within the conscious organism.

32. With the emergence of the generic *qualia* of conscious enjoyment, developed only in organisms which possess certain privileged nerve-centres, we have the enjoyment of *abc*-process. Can we say in what respect this *abc*-process, even in the earlier phases of its development, differs from *ab*-process, otherwise than in virtue of the emergent *qualia* of its enjoyment? Tentatively we may say that prospective value has been supplemented by prospective awareness or what I have termed pre-awareness.

33. Prospective awareness or pre-awareness is representative not presentative of external occurrences. When *x* is aware of *y*, and *y* of *x* (Note 2), the awareness is presentative in the sense of being direct and immediate. Actual compresence is essential. So too the unconscious organism—*e.g.* presumably the plant—is aware only of that with which it is actually compresent, in the sense of being subject to the direct influence of that of which it is then and there aware.

34. But the conscious organism is not only aware of external occurrences with which it is thus actually compresent, it has also a foretaste of occurrences with which—as interpreted by conceptual thought—it will be compresent. Stress is here laid on the “will be” rather than the “has been” on genetic grounds. Prospective awareness in the conscious organism is the supplement of prospective value in the as yet unconscious organism. But in neither case is explicit reference to the future necessarily implied by the phrase. Explicit reference to the past or the future,—the past in origin, the future in application,—comes only with conceptual thought (Note 43).

35. Both prospective value and prospective awareness, however, each after its kind, are founded on (1) repetition of routine, and (2) retention, in later phases of that routine, of changes wrought in earlier phases. Where prospective value alone obtains there is reaction only to presentation of the compresent; there is no provision for representation of the not yet compresent.

36. What then is the nature of this provision, where *abc*-process has been established? Again the answer must be tentative. The cortex may be regarded as a system of

centres α , β , γ , etc., each connected with others *via* neurones, but partially isolated from others by synaptic resistance. Let the *ab*-process and enjoyment in one of these, say δ , be changed through awareness, *via* extero-receptors, of an occurrence external to the organism. Of that change other centres in the cortex are physically aware *a*-fashion; but physiological awareness, *ab*-fashion, seems mainly to be through neuronic connexion.

37. Let two such centres, say δ and σ , have their process and enjoyment modified in succession, at not too great an interval, through the awareness of routine occurrences in the external world, successively compresent with them. Then the synaptic resistance in the neuronic connexion between δ and σ is overcome, and is thenceforward lessened through retention—synaptic resistance towards β , λ , τ , etc., remaining undiminished.

38. Hence, if the routine be subsequently repeated, on the recurrence of similar process in δ , through awareness of the correspondent occurrence in the external routine, σ -process is revived owing to the lessened synaptic resistance in the neurones connecting δ and σ . This revival, as representative pre-awareness, precedes the presentative awareness through sense-receptors. Thus on lifting a cup of coffee to our lips we have pre-awareness of the coming taste before the taste-receptors are stimulated.

39. A routine in the nerve-centres is called forth by the routine in the external world. Traces left by former routine form physiological dispositions for later routine. And, in revival, the representative sequence of pre-awareness outstrips the sensory sequence with its direct presentative awareness of external routine. This outstripping is essential to render pre-awareness of any service. In pre-awareness prospective value is raised to a more effective level.

40. It must be remembered that the sensory centres of the cortex are also connected, *ab*-fashion, by neuronic enchainment, with cortical and sub-cortical motor-centres, and through them serve to control behaviour. Essential as is this control in the adjustment of routine in an active organism to the inter-woven routines of its environment, one essential feature alone demands emphasis here.

41. It is a necessary condition of all conscious control that it should emanate from centres in which there is representative pre-awareness. All profiting by experience is profiting by prospective awareness. All conscious control is preventive in the older sense of the word.

42. Presentative awareness of what is then and there

compresent in space with the organism will not suffice. If it is already compresent in such wise as to call forth direct sensory awareness, there it is, and it cannot be escaped or avoided; and if it is not yet thus compresent it can call forth no direct awareness. No doubt there is a sense in which it may be said that to be of any avail, prospective meaning is compresent with the sensory presentation which it qualifies. But that is not the sense in which I here use the word. What I urge is that prospective awareness, as such, *has reference to* that which is not yet compresent in such wise as to afford direct awareness; and that conscious control must always involve some forestalling of an event which is coming but has not yet come.

43. Now although it is impossible for us to *interpret* either pre-awareness or prospective value save in reference to coming events, it is probable that, when prospective awareness first dawns on the scene, there is therein no explicit reference to the future. Pre-awareness, as a form of meaning, just conspires with direct awareness to modify in a distinctive manner the totality of enjoyment. No doubt this distinctive manner is that from which explicit reference to the future is in due course developed. But in perceptual life the emphasis is on the modification of enjoyment here and now. At most there is, born of routine, a vague "fringe of comingness" attaching to the present enjoyment, such as we have with regard to the further position of hand and arm in the middle of a stroke in billiards, before the impact of the cue on the ball.

44. But if, in accordance with what was suggested in Note 38, δ , as a factor in the totality of *ab*-process in the cortex, is the main determinant of a change in σ , partially reinstating therein a change like to that which had previously been determined *via* extero-receptors, the reference from σ , as a centre of enjoyment, is normally to the object which directly stimulates δ , and which will, perhaps, a little later directly stimulate σ .

45. The whole problem of reference needs further and fuller discussion than, I think, it has yet received. It seems that where there is a chain of transeunt awareness proceeding inwards, say from candle-flame to retinal receptors, from them to lower nerve-centres, and from them again to our cortical centre δ , the reference from enjoyment outwards skips all intermediate links of the chain and goes straight out to the candle-flame, the light of which we see.

46. If then subsequently the functioning of δ is determined, not immediately by stimulation of the retina but mediately

by intra-cortical process, the reference of the enjoyment in δ is still what it was in the case of the initially immediate awareness—namely to the object. It must be remembered, however, that we have been dealing only with factors in the totality of enjoyment, which as a totality is a complexly integrated whole with additive and constitutive characters. We must remember, too, that what we come to interpret as its reference is a reference from this unified enjoyment as a whole to some in like manner unified psychological object set in a context of reference. But I cannot here follow up this topic. It is time to bring these Notes to a close.

47. I have, in that which precedes, endeavoured to restrict my speculations with regard to the evolution of enjoyment through awareness, to intra-mundane processes within the order of nature. I have dealt with emergent *qualia* as I conceive them to come into being, basing acceptance of their existence on what I regard as empirical evidence so far as it can be obtained.

48. There is, however, nothing in what I have said which precludes the belief in an extra-mundane Source of existent process and of the successively emergent *qualia*. Metaphysical supernaturalism, if it be accepted, is supplementary to, and nowise antagonistic to, naturalism.

49. If a supernatural Source be accepted it will be held to comprise in its being—*eminenter*, as the Schoolmen would say—all known *qualia* and such others as may hereafter be manifested through its operation. And the highest *qualia*—those of the tertiary order including spiritual values—may be metaphysically regarded as those manifestations within our enjoyment through which we are in closest attainable touch, in our present life, with Spiritual Agency.

50. It is clear, however, that whereas naturalistic interpretation must proceed upwards, as we have done, on the principle that the higher implies, as logically prior, the lower, the course of metaphysical explanation will run in the opposite direction. All manifestations, even the lowest, imply a Spiritual Source in the eminent fulness of its being.

II.—ROUSSEAU'S DOCTRINE OF THE RIGHT TO BELIEVE.

BY NORMAN WILDE.

To admit the truth of Mme. de Staél's judgment on Rousseau that "il n'ait rien inventé mais tout enflammé," is not to admit its whole truth. Like all great preachers he was conservative in doctrine, his power lying in his appeal to the instinctive beliefs to which he called men back, rather than in any novelty of idea. He was not an innovator but a reformer. The ideas with which he inflamed the hearts of his generation were the primitive beliefs of the simplest domestic pieties, pieties the value of which had been ignored by the cultured of his day, but the practice of which had been part of the common life of the race.

But though a contemner of reason and a foe to the *philosophes*, Rousseau has yet his place in the history of thought. If his beliefs were old, the ground upon which he justified them was relatively new. In his passionate defence of these beliefs which to him were life, we have one more phase of the perennial protest raised by faith against the limits of thought. Forbidden by the philosophy of his day to cherish even the meagre hopes implied in the most attenuated of Christian theologies, and unable to find in even the most religious of systems sufficient upon which to build his faith, he abandoned reason and found in feeling the organ of religious truth. In this emphasis upon feeling and the right to believe he seems to anticipate the tendencies of the present, but so unsystematic is his teaching that, in spite of the studies his bi-centenary has evoked, his doctrine of belief seems not yet clearly defined. To understand that doctrine it is necessary to take into account both the content of his beliefs and their relation to his temperament.

The dominant factor in Rousseau's thought is his optimism, not the pedantic optimism of popular theology extracted from Leibnitz and ridiculed by Voltaire, but an involuntary, temperamental optimism, the expression of his incurably romantic temper. Sensitive and dreamy from a child, he had developed the habit of ecstatic enjoyment

of solitary nature. The world as felt in these deliciously delirious moments was for him the real world. The miseries of life, keenly as he felt them in his own experience, seemed only to drive him more surely into this world of his dreams, and to fix in him more firmly his belief in its reality. Against such revelations of the beauty and goodness of life, the mere facts of evil were of little avail. The Lisbon earthquake, so shattering to the complacency of Voltaire, left Rousseau unshaken. If men had been living as nature intended them to live, they would not have had such lofty houses to be shaken down about their heads, or such cities to be destroyed, he suggests. But it is not on facts that his optimism rests. It is not a matter of hedonic induction by which the various happinesses of the world are pieced together into the vision of a perfect whole. On the contrary, it is the vision of the perfect world that makes radiant its parts. "Au lieu de tout est bien, il vaudrait peut-être mieux dire le tout est bien, ou tout est bien pour le tout."¹ In other words, belief in God is the condition, not the result, of finding life good. Rousseau makes some attempt to argue the problem of evil in historic ways, but such arguments have little interest for him. His own emotional certainty is sufficient.

This optimism becomes explicit in his belief in God. Indeed, it is hardly to be distinguished from it. This gloriously beautiful universe with which he is in love can only be thought by him as the expression of a personal will and the embodiment of a moral purpose. "This being who wills and can perform his will, this being active through his own power, this being whoever he may be, who moves the universe and orders all things, is what I call God. To this name I add the ideas of intelligence, power, will which I have brought together, and that of kindness which is their necessary consequence, but for all this I know no more of the being to which I ascribe them. . . . I see God everywhere in his works; I feel Him within myself; I behold Him all around me; but if I try to ponder Him Himself, if I try to find out where He is, what He is, what is His substance, He escapes me and my troubled spirit finds nothing."² This indefiniteness of conception allows Rousseau to combine a double set of attributes in his idea of God, the naturalistic and the moral. By heredity and training he is a Genevan Protestant and a worshipper of the just God of Calvin, whose existence is the corner-stone of the moral order. This God is a transcendent God, an essentially personal God, whose

¹ Letter à Voltaire, 18th August, 1756.

² *Emile*, Book IV.

will is the standard by which we distinguish the evil from the good. It is this God whom Rousseau preaches in the *Emile* and confesses in his *Letter to the Archbishop of Paris* and *Letters from the Mountain*. It is by virtue of this faith that he can still profess himself a Christian, though banned by Geneva and Rome. But the God of his experience is of by no means so definite and austere a character as this. The object of the raptures of the sentimental nature worshipper is vague and ill-defined. He feels, he feels intensely, but what he feels he does not know. He is carried out of himself and lost in a delicious sea of being. Thought and will give place to emotion and the distinctions of subject and object are lost. Though the experience may be afterwards interpreted as the worship of the Christian God, in itself it is without form and void. Of this divine reverie Rousseau gives us many glimpses; here is one of the less emotional and more attractive, his morning worship at Isle Saint-Pierre: "Immediately I rose from my bed, I never failed, provided the weather was auspicious, to run to the terrace to breathe the fresh and wholesome air of the morning. . . . I know no homage more worthy of the divinity than the silent admiration excited by the contemplation of his works. . . . It is especially at rising, wearied by a want of sleep, that continual habit inclines me to this elevation, *which does not impose the fatigue of thinking*. But to this effect my eyes must be struck with the ravishing views of nature. In my chamber I pray less frequently, and not so fervently; but at the view of a beautiful landscape I feel myself moved, by what power I am unable to tell. I have read somewhere of a wise bishop, who, in a visit to his diocese, found an old woman whose only prayer consisted in the single interjection 'Oh!' 'Good mother,' said he to her, 'continue to pray in this manner. Your prayer is better than ours.'¹ This better prayer is mine also." In experiences such as these God is but the name which he gives to the stimulus for these transports of being. The beauty and wonder of the world mean God.

Beyond the assertions that God is the prime mover of the world, the basis of its goodness, and the source of its beauty, Rousseau professes himself unable to go. He cannot say that his nature is like man's, that he is the sole principle of reality, or that he has created the world. His functions can perhaps best be epitomised as being those of the principle of order in the universe. Rousseau's religious emotions are not those of love, but of admiration. He seeks no favours,

¹ *Confessions*, XII.

he asks no love in return, he is contented to know and feel the perfect harmony of the world.

But while on its naturalistic side the religion of Rousseau sometimes suggests the "amor intellectualis dei" of Spinoza, on its moral side, it shows marked contrasts. The second point in his doctrine is the immortality of the soul and the certainty of rewards and punishments after death. Here we have the position later to be formulated more completely by Kant. The justice of God is invoked for this tenet. Having made man sensitive and having laid on him the duty of virtue, God cannot in justice leave his legitimate demands unsatisfied. Rousseau, like Kant, had no intention of saying that happiness is the reward and sanction of virtue, but only that it is its reasonable completion. His attitude is that of the Psalmist grieved at seeing the wicked flourishing like the green bay tree. "Had I no other proof of the immaterial nature of the soul, the triumph of the wicked and the oppression of the righteous in this world would be enough to convince me. I should seek to resolve so appalling a discord in the universal harmony. . . . I do not say that the good will be rewarded, for what greater good can a truly good being expect than to exist in accordance with his nature? But I do assert that the good will be happy, because their maker, the author of all justice, who has made them capable of feeling, has not made them that they may suffer. . . . This feeling relies not so much on man's deserts as on the idea of good which seems to me inseparable from the divine essence. I only assume that the laws of order are constant and that God is true to Himself."¹

This soul of man is also only to be thought as free. "I am only aware of will through the consciousness of my own will, and intelligence is no better known to me. When you ask me what is the cause which determines my will, it is my turn to ask what cause determines my judgment; for it is plain that these two causes are but one; and if you understand clearly that man is active in his judgments, and that his intelligence is only the power to compare and judge, you will see that his freedom is only a similar power, or one derived from this. He chooses between good and evil, as he judges between truth and falsehood; if his judgment is at fault, he chooses amiss. What then is the cause that determines his will? It is his judgment. And what is the cause that determines his judgment? It is his intelligence, his power of judging: the determining cause is in himself. Beyond that, I understand nothing."² This freedom is thus a spiritual

¹ *Emile*, IV.

² *Ibid.*

spontaneity which distinguishes man from the animate, as well as inanimate, world. It does not involve indifference toward the good, but the absence of external constraint in the choice or rejection of it. It means that his choice is his own and not the effect of an external cause. That the root of the act lies in the judgment rather than in a non-intellectual factor, does not for him affect the moral significance of the choice, for man is responsible for the right use of his judgment. To his wrong choices is due all the evil of the world. "Evil in general can only spring from disorder, and in the order of the world I find a never-failing system. Evil in particular cases exists only in the minds of those who experience it, and this feeling is not the gift of nature, but the work of man himself. Pain has little power over those who, having thought little, look neither before nor after. Take away our fatal progress, take away our faults and our vices, take away man's handiwork, and all is well."¹ The intellectualism of this is perhaps only in apparent conflict with his earlier famous saying, "l'homme qui médite est un animal dépravé".²

This spontaneity of the mind he illustrates further in his theory of knowledge. The fundamental fact is the experience of the self in its sensations. The sensations are of objects which are to be conceived as external to the self and causally related to its sensations, which arise independently of the will. Whether these external objects are themselves ideas or not, is not important; they are at least another than the self. "Through sensation objects present themselves to me separately and singly as they are in nature; by comparing them I rearrange them, I shift them so to speak, I place one upon another to decide whether they are alike or different, or more generally to find out their relations. To my mind, the distinctive faculty of an active or intelligent being is the power of understanding this word 'is'. I seek in vain in the merely sensitive entity that intelligent force which compares and judges; I can find no trace of it in its nature. . . . This power of my mind which brings my sensations together and compares them may be called by any name; let it be called attention, meditation, reflection, or what you will; it is still true that it is in me and not in things, that it is I alone who produce it, though I only produce it when I receive an impression from things. Though I am compelled to feel or not to feel, I am free to examine more or less what I feel."³ Here Rousseau breaks

¹ *Emile*, IV.

² *Discours sur l'inégalité*.

³ *Emile*, IV.

squarely with Helvétius and ranges himself with "the illustrious Clarke" and the English rationalists.

So, too, in his theory of conduct, the spontaneity of the individual is his principle. There is in man, taken by himself, a native tendency to self-expression and self-preservation, *amour de soi*. This tendency, since man is by nature good and reasonable, is identical with the love of order and justice, and is the root of all the virtues. Had man but remained in the state of nature in which he was created, his goodness would have been but the natural unfolding of a flower in a peaceful garden. Having made the fatal step of organising a social life, however, this same self-love became the root of all the evils. Man learned to compare himself with his fellows, and, with his consciousness of inequality, came pride, envy, and ill-will. Instead of *amour de soi* was *amour propre*. Yet still in our corrupt condition there remains to us the original voice of reason bidding us express our true nature and realise justice in the world. Morality is thus neither a seeking of pleasure nor obedience to law, but the self-expression of a nature fundamentally good.

Rousseau's optimism thus penetrates his whole creed: a benevolent God, who has created a marvellously beautiful world, in which He has placed noble and generous men, who, through the exercise of their wholly desirable freedom, have brought upon themselves evils, which, however, are bound to be redressed in a life beyond the grave, to the reality of which the justice of God is pledged.

If we ask where Rousseau found this optimistic world view, the answer is not far to seek. In its outlines it was identical with that rationalised Christianity then known as Deism and now prevalent as liberal Protestantism. It was Christian Theism minus the doctrines of the fall and redemption. "Born in a family where morality and piety reigned, educated with kindness by a minister full of wisdom and religion, I received from my tenderest childhood principles and maxims, others would say prejudices, which have never entirely left me."¹ Under the influence of Mme. de Warens and nature, he became "*dévot presque à la manière de Fénelon*". Instinctively averse to authoritative dogma, his reading of philosophy and his first intercourse with the sensationalists and naturalists at Paris developed his naturalistic tendency and helped pare down his inherited Christianity. Repelled by the anti-religious extremes of consistent sensationalism he seems to have fallen back upon the

¹ *Rêveries*, III.

English rationalists and sentimentalists, especially Clarke and Shaftesbury, to the former of whom he refers as "the illustrious Clarke who gives light to the world and proclaims the Being of beings and Giver of things,"¹ whose system, so simple yet so great, seems to him freer from contradictions and difficulties than any he has found. The influence of the sentimental optimism of Shaftesbury, known to him through Diderot's translation of the *Inquiry*, is evident throughout his moral theories as well as in his theology.² For the positive contents of his creed, therefore, Rousseau seems to have been indebted to these English believers in a rationalised Christianity. Even for the enthusiasm with which he held it, faint parallels may be found in the Platonicising theologians and in Shaftesbury. But the life and spirit of it were his own, and it was these that made it the living force it was for his generation.

Turning now from this inherited content of his faith to that part of his teaching which is more peculiarly his own, we come to the problem of logical method and the basis of belief. And perhaps first it is well to note that Rousseau believes that he had a method. He tells us in his *Rêveries*³ that he had reached the age of forty before he had attained any rational principles of living. Up to that time he had drifted at the mercy of chance influences, distracted from his duties "without scorning them but often without rightly knowing them". In his youth he had fixed upon that age as the limit of his drifting, and now that it had arrived, he withdrew from the world, changed his habits, his costume, and his friends, and thought out for himself a system of principles that might serve him for the rest of his life. These, once adopted after the most earnest investigation, he believed that he ought not again to subject to criticism since he could not hope ever again to be in a better position to test their truth. Constant revision would mean growing uncertainty and practical instability, a condition incompatible with the best conduct of life. Whether it was his years at the Hermitage and at Montmorency, issuing in the publication of his three main works, the *Contrat Social*, the *Nouvelle Héloïse*, and the *Emile*, to which he refers, or the period following the first *Discours* is not quite clear. At any rate, he gives us to understand that, following the

¹ *Emile*, IV.

² For Rousseau's debt to English thought, cf. Ch. Borgeaud, *J. J. Rousseau's Religions-philosophie*, i., 2. Cuendet, *La phil. religieuse de J. J. Rousseau*, I., iii., 111.
Rêveries, III.

example of Descartes, his opinions were the result of systematic doubt and the adoption of a conscious method. To this account it is not possible to give full credence. As usual, Rousseau has read back into his motives the reasons which justified the results. But though we have to explain his retirement and philosophic activity by motives more human than youthful resolve, it is true that in his fifth decade he was forced, in self-defence, to examine the grounds of his instinctive beliefs and to justify them before his world. That his doubt at this time extended beyond the proofs of his beliefs to the beliefs themselves is far from likely. Rousseau's temper was too strongly emotional and too little intellectual to subject him to any agonies of doubt. "They had not persuaded me," he writes of his former friends, "but they had disquieted me. Their arguments had shaken me, without ever convincing me. I could find no good reply but I felt there must be one. I accused myself of unskillfulness rather than of error, and my heart replied better than my reason."¹ His task during these critical years, therefore, consisted in making clear to himself the grounds of his faith, in finding, as Mr. Bradley would say, bad reasons for what he believed upon instinct.

And in the first place it is significant that Rousseau does offer reasons for his beliefs. He has no intention of dividing human nature sharply against itself, and relying upon instinct to the exclusion of reason. His inclination seems rather to go with reason as far as it will carry him and then in its extremity take refuge in feeling. As in orthodox scholastic circles, reason is a good propadeutic to faith. And so in the *Profession of Faith* in the *Emile*, we find the usual rationalistic proofs for the existence of God, based upon the natural inertia of matter and the necessity for a Prime Mover, whose intelligence and benevolence are implied in the order and goodness of the world. It might be the voice of Newton or Clarke we hear in these physical considerations, adduced for the support of this theological tenet. So, too, in the discussion of the nature and immortality of the soul, while it is the moral argument upon which he ultimately relies, he is at pains to make clear the metaphysical point as to the natural distinctness of soul and body and the consequent possibility of their separation. Nor is there any suggestion that these reasonings are not valid or even that their validity rests upon their utility. He seems as dogmatic as the rationalistic theologians.

But in the next breath his voice may be raised against

¹ *Rêveries*, III.

reason and its friends, especially against its friends. No term is too harsh for him to use about the members of the materialistic group of whom he was once the associate. Any doctrine advocated by them was for Rousseau suspect. The frivolity and insincerity of their lives vitiated for him their teachings. "Their passions which governed their doctrines, their interest in making this or that believed, rendered it impossible to discover what they themselves believed. Can one look for good faith among party leaders?"¹ Conscious of the part played by the passions in his own life, he is inclined to demand moral integrity as the condition of intellectual leadership, if not of the attainment of truth itself.

Whether this distrust of the pronounced votaries of reason in his own circle had anything to do with it or not, we find him advocating a conception of belief in which reason plays a minor rôle. Undoubtedly the main explanation of his position is to be found in the felt inadequacy of reason to prove that which he believed. He could not but realise that that perfect world which was the object of his enthusiasm, and that moral order which was the standard of his life, were not capable of demonstration by any logical processes known to man. The severe and barren theodicy of the rationalistic deist might rest on such a logical basis, but not the emotionally satisfying system of Rousseau. Hence after having discussed the attributes of God he concludes: "If I have succeeded in discerning these attributes of which I have no absolute idea, it is in the form of unavoidable deductions, and by the right use of my reason; but I affirm them without understanding them, and at bottom that is no affirmation at all. In vain do I say, God is thus, I feel it, I experience it, none the more do I understand how God can be thus. In a word, the more I try to envisage His infinite essence, the less do I comprehend it; but it is, and that is enough for me; the less I understand the more I adore. I abase myself saying, 'Being of beings, I am because Thou art: to fix my thoughts on Thee is to ascend to the source of my being. The best use I can make of my reason is to resign it before Thee; my mind delights, my weakness rejoices, to feel myself overwhelmed by Thy greatness.'"² And in his letter to Voltaire he writes: "I admit frankly that neither the affirmative nor the negative seem to me demonstrated by the light of reason alone, and that if the theist only finds his faith on probabilities, the atheist still less

¹ *Rêveries*, III.

² *Emile*, IV.

exact founds his on the contrary probabilities.”¹ And again he complains that men are “ignorant of just that which it is the most important for them to know, man. We see neither the soul of another, because it is concealed, nor our own, for we have no mirror of the mind. Born blind, we have no conception of sight and, unconscious that any faculty is lacking, wish to measure the limits of the world, though our short telescopes, like our hands, reach but two feet before us!”² “The jargon of metaphysics has never led to the discovery of a single truth, and it has filled philosophy with absurdities of which we are ashamed as soon as we strip them of their long words.”³ And in a letter to Moulton he explains that he has used this “jargon” because the materialists, whom he wishes to refute in the *Profession of Faith*, understood no other.⁴ His conclusion of the matter is: “Sans le sentiment interne, il ne resterait bientôt plus traces de vérité sur la terre”⁵.

But when we come to consider what Rousseau offers as substitute or supplement for reason, exact statements fail us. That there is an organ of religious and moral truth other than reason he asserts freely, but what it is and what are its relations to reason, he nowhere clearly says. The names for it are various, *sentiment, sentiment interne* or *intérieure, conscience, lumière naturelle, instinct moral*. By these terms he means to indicate a kind of immediate and infallible source of truth native to man and free from the possibility of error inherent in the reasoning processes. By this means man comes in contact with super-sensible reality and is furnished with the spiritual facts upon which his moral and religious life is built. Although these facts may not be explicable or reducible to system or even consonant with ordinary scientific views, they have a certainty and necessity which makes them the fixed points in any complete philosophy of life. Against these rocks the waves of rational scepticism and dogmatism beat in vain.

As applied to moral truth, the term conscience is the usual one, and we have the orthodox doctrine of its infallibility. He defines it as “an innate principle of justice and virtue”. “Too often does reason deceive us; we have only too good a right to doubt her; but conscience never deceives us. She is the true guide of man; she is to the soul what instinct is to the body; he who obeys his conscience is following nature

¹ *Lettre à Voltaire*, 18th August, 1756.

² *Lettres sur la vertu et le bonheur*.

³ *Emile*, IV.

⁴ *Lettre à Moulton*, 1st August, 1763.

⁵ Cf. Cuendet, *op. cit.*, ii., 1. *Le scepticisme de Rousseau*.

and need not fear that he will go astray."¹ And yet although he claims infallibility for it, he insists that "the decrees of conscience are not judgments but feelings. Although all our ideas come from without, the feelings by which they are weighed are within us, and it is by these feelings alone that we perceive the fitness or unfitness of things in relation to ourselves, which leads us to seek or shun these things."² "To know good is not to love it; this knowledge is not innate in man; but as soon as his reason leads him to perceive it, his conscience impels him to love it. It is this feeling which is innate."³ In a note he tries to minimise this distinction between idea and feeling by pointing out that it is a matter of emphasis, "when we are chiefly concerned with the object and only think of ourselves as it were by reflection, that is an idea. When, on the other hand the impression received excites our chief attention, and we only think secondarily of the object causing it, it is a feeling." Conscience is thus the instinctive emotional reaction to situations the nature and meaning of which are learned from experience. It is not the imperative of Kant or even the intuition of the neo-Stoics, but the moral sense of Shaftesbury.

But this organ of truth is not merely moral in its function, it is the source of an ultimate world view. By it we know that God is, and that there is a future life of rewards and punishments. Here the theory meets with more difficulty. It is not so hard to conceive that there is an instinctive reaction by which we respond to the value for us of various situations, our satisfactions and dissatisfactions indicating the real worth of conduct, for the world of values seems naturally related to our feelings, but when these same feelings are made the basis for belief in an objective order, the matter becomes not so clear. Apparently, as in the case of the conscience, strictly so called, he does not mean to assert the power of the feelings to give us new ideas for he recognises clearly enough that his religious ideas have had a history and are due psychologically to the associations of his childhood and youth.⁴ This sentiment, therefore, must be also a subjective evaluation or test of the truth of ideas elsewhere derived. It is our emotional touchstone by which we distinguish the true gold from the false. Hesitating between theism and atheism, finding no compelling objective reason for decision, the strong emotional value of the former forces it upon our acceptance with an intensity and intimacy which are a substitute for reasons. It ought to be true therefore

¹ *Emile*, IV.

² *Ibid.*

³ *Ibid.*

⁴ *Rêveries*, III.

it must be true. It fits in with the ideas we cherish, and gives organic unity to life. Lost in rapture at the vision of the divine there can be no question of doubt for Rousseau: "Quand je veux m'élever à lui je ne sais où je suis; n'apercevant aucun rapport entre lui et moi, je ne sais pas où l'atteindre, je ne vois ni ne sens plus rien, je me trouve dans une espèce d'anéantissement."¹ It is not a case of his accepting an idea but of being possessed by it.

Granted, then, that the feelings are not a revelatory, but a ratifying, faculty, there comes the question of validity. And here there meets us the problem of the pragmatism of Rousseau. In spite of the fact that the ambiguity of the term makes the use of it relatively safe, it also makes it undesirable to apply it without care. Recently Prof. Schinz² has included Rousseau among the objects of his anti-pragmatism crusade and has been followed in his classification by Cuendet and Hibben.³ That there is some ground for this classification is beyond doubt. Rousseau by temper and inclination is true to the type. The theory would have found him a willing convert, offering him salvation from the mechanical logic of his time. But to recognise that this modern theory of truth would have answered his needs and clarified his thought, is not to attribute it to him. The distinctions in the history of thought are as real as the continuity and a doctrine of preformation is no more useful in history than in biology. While Rousseau, therefore, exhibits points of agreement with James and Schiller, his points of difference are equally vital.

In the first place he agrees with them in his emphasis upon useful knowledge. Emile is to be guarded from useless science: "The question is not to know what is, but only what is useful".⁴ "I am content if he knows the 'wherefore' of his actions and the 'why' of his beliefs. For once more my object is not to supply him with exact knowledge, but with the means of getting it when required, to teach him to value it at its true worth, and to love truth above all things."⁵ These passages Prof. Schinz cites as evidence that truth for Rousseau means "practical truth" in opposition to science. It is evident enough that he is drawing such a contrast here, but the slightest acquaintance with the remainder of the book should suffice to show that far from identifying truth in general with useful truth, such utilities are for him but fragments of the great body of knowledge

¹ *Nouvelle Héloïse*, v., 5.

² *The Monist*, 1909.

³ Cuendet, *op. cit.*, i., 11. Hibben, *Phil. of the Enlightenment*, p. 157.

⁴ *Emile*, III.

⁵ *Ibid.*

from which must be carefully selected the parts suitable for youth. As the former quotation implies, there *is* a knowledge of what is, apart from what is useful, though it is not for the child and rarely for the man. The doctrine is educational not logical. Were we to take such practicality as evidence of pragmatic logic we must accuse most of the moralists of the age of Queen Anne.¹ That he agrees with Pope as to the proper study of mankind implies that there are other studies which the unwise may improperly pursue, improperly, not because they do not lead to knowledge but because they do not lead to virtue. So too, his comment on himself that "I have never seen him listen calmly to any theory that he believed harmful to the public weal,"² refers to no theory of truth but indicates the primacy of his moral interest or the hastiness of his moral prejudice. To the same effect is his statement: "I know only that the truth is in the things and not in my mind which judges them, and that the less I put of my own in my judgments about them, the surer I am to come near the truth: thus my rule, to listen to sentiment more than to reason, is supported by reason itself".³ Whatever we may think about the consistency of this testimony of reason against itself, it is clear that Rousseau has in mind no doctrine of subjectivity here but is appealing to an intuitive sentiment which, as giving immediate contact with an objective order, is more reliable than those mediate processes which are liable to be vitiated by the disturbing effects of conscious purpose. The mind does not create or form truth, it finds it, and its organ is this feeling or intuitive reason.⁴

In the second place, his doctrine shows pragmatic colouring in the place given to the feelings as grounds of belief. Not merely do they determine our judgments, but they have a right to do so. Passages could be multiplied in illustration of this contention: perhaps the following are typical. "You say that my reason chooses the sentiment that my heart prefers, and I do not deny it. That is what happens in every deliberation where the judgment has not enough light to decide without the help of the will."⁵ With reference to the immortality of the soul he writes: "As this assumption is consoling and in itself not unreasonable, why should I fear to

¹ Cf. J. Texte, *J. J. Rousseau and the Cosmopolitan Spirit in Literature*.

² *Dialogues*, II.

³ *Emile*, IV.

⁴ Cf. Parodi, *La phil. religieuse de J. J. Rousseau, Revue de Métaphysique et de Morale*, 1912.

⁵ *Dialogues*, II.

accept it?" "All the subtleties of metaphysics would not make me doubt the immortality of the soul and the reality of a benevolent Providence. I feel it, I believe it, I wish it, I hope it, and I will defend it to my last breath."¹ And in his reply to the Archbishop of Paris he suggests that some day, when wearied of fruitless discussions about creeds, men will come together in an assembly from which theologians would be excluded, and will agree upon the creed most useful to man for "We may by this means hope to approach as near to the truth as possible to men; for we may assume that what is most useful to the creature of his hand is most agreeable to the Creator".² "The inner proof or that of sentiment is the only one which can render all the others invincible."³ And the Vicar's final appeal to Emile begins: "My son, keep your soul in such a state that you will always desire that there should be a God and you will never doubt it."⁴

Confining our attention to passages such as these, and ignoring those in which the belief is one which concerns values, for here Rousseau recognises that "La vérité morale n'est pas ce qui est, mais ce qui est bien," we find that while his doctrine is almost identical with that of the will to believe, it is not pragmatic. What he is contending for is just that which James explained was his own real object, the right to believe in cases where objective and conclusive evidence is lacking. So far is he from ignoring the existence and value of such objective grounds that his own position was assumed only after critical, or would-be critical, examination of such evidence. He admits: "I do not doubt, it is true, that the prejudices of childhood and the secret longings of my heart have weighed down the balance on the side most comforting for me. It is hard to keep from believing what one desires with so much ardour, and who can doubt that the interest for or against the judgments as to the future life decides the faith of most men as to their hopes and fears? All that might fascinate my judgment, I am sure, but not affect my good faith, for I feared deceiving myself on every point." The issue being the grave one of risking eternal life for the pleasures of the world, it was necessary to reach some conviction. Unable to solve all the mysteries involved, "I adopted in each question the belief which seemed to me the best established immediately, the most credible in itself, without stopping for objections which I could not resolve, but which were confuted by other objections not less strong in the opposed system".⁵

¹ *Lettre à Voltaire.*

² *Nouvelle Héloïse*, V., v.

³ *Lettre à Ch. Beaumont.*

⁴ *Emile*, IV.

⁵ *Rêveries*, III.

Whether this account of his own procedure is biographically correct or not, there is no doubt that it gives the key to the interpretation of his various statements of doctrine. However slight his interest was in pure science he never meant to substitute for it faith. So far as an objective procedure could go he was willing to go with it. But when the path of reason lost itself in a confusion of issueless alternatives he claimed the right of committing himself to this "instinct of the soul". He took no pains to mark out the limits of reason as did Kant, nor did he, as James, define the conditions under which it was reasonable to exercise the right to believe, but his thought is similar; there is a field for reason but it is limited, and there are conditions under which we must believe without proof. Those conditions, as implied in the passage just quoted, are a vital issue, a living option, and the silence or contradictions of reason. In such a situation it is the part of wisdom to fall back upon that which commands itself immediately to our natural feeling. It is a case of rationalist rogues falling out and honest men getting their due. In Rousseau's conception this honest common-sense nature which comes to its own expresses itself in the form of an emotionally welcomed intuition, rather than in the will, as in the doctrine of James. His emphasis is upon the feeling of the truth or the seeing of the fact, rather than upon the legitimacy of the conduct based upon an assumption. The right to cherish a lovely vision, rather than to lead a strenuous life, is that for which he contends, to be a hearer, rather than a doer, of the word.

The passage in the letter to the Archbishop of Paris seems to suggest, perhaps, more of a pragmatic tendency than has been admitted, but when we consider its context it loses its significance. Far from discussing the nature of truth, Rousseau is here concerned with the question of external unity in public worship, with *la religion civile*, as he calls it in the *Contrat Social*. A pronounced individualist in personal religion, he recognises the necessity for, and at the same time the difficulty in attaining, some unity in the religious life of a people. The state cannot tolerate every variety of belief. Unable to reach a common body of doctrine upon rational grounds, the only resource is to agree upon a system useful to the state, in the belief that the promotion of the welfare of man is the object of the divine will. The problem is a purely practical one, essentially a political rather than a philosophical matter. That Rousseau believes that the useful doctrine is true, is quite probable, but he has no intention here of making its utility the essence of its truth.

On the contrary, the implication is rather that there is an objective order of truth to which we can at best but approximate. As good citizens it is the part of contentious sectaries to lay aside their private dogmas and realise the true end of civic religion, the welfare of the state.

Nor in his advice to Emile, does Rousseau mean to found belief in God upon desire. To desire the existence of God means for him to be in a state of mind free from pride and self seeking and accordingly more ready to recognise the objective truth revealed in feeling. "The silence of the passions" is for him an indispensable condition for the reception of truth, and it is this that is implied in the desire for the existence of God. The real ground for that belief would be not in the desire but in the inner voice which makes itself heard in this silence, in the conviction felt when nature has had a chance to speak.

If now, we ask what ground there is for believing that these intuitive convictions are true, we are thrown back again upon Rousseau's fundamental optimism. Human nature, as the product of a good God, is essentially and primitively good. Our original instincts and cognitive data are given by God and are adapted by him to our practical and cognitive needs. If we can only free ourselves from the disturbing influences of civilisation, strip ourselves of the accretions of error due to our wilful mistakes, and regain something of the simplicity of vision with which we were originally endowed, we may hope to attain truth. It is this optimistic prejudice which lies at the root of the popular Stoic and even Christian doctrine, that gives to immediate feeling its weight with Rousseau. The ideas thus accredited are not man-made, but are part of the universal heredity from God.

The apparent circle involved in making belief in God depend upon feeling and feeling depend for its validity upon belief in God, means that Rousseau has reached his ultimate here. His optimism, or his belief in God, is his primary postulate, rooted in his emotional temper. It has no logical ground but only psychological fixity. Accepting it as he must, its existence and validity then seem natural upon the basis of its content. Like Descartes, he feels that whatever has the same kind of certainty, as this idea of God, must be true, for God would not deceive him, only with Rousseau the certainty is one of feeling and not of insight. This optimistic postulate he does not make explicit in his logic, but it lies at the root of his thinking not only in religion, but in politics, social theory, and education. Thinking is

one of the artificialities of life, one of the luxuries of a corrupt civilization, and as such its results are less trustworthy than the simple intuitions of a state of nature.

If, then, we understand by pragmatism a general theory of knowledge which denies the objectivity of truth, and places its essence in its utility, we must refuse to identify with it this theory of Rousseau. At the same time we must recognise his likeness in personal temper to pragmatists of the type of James, a likeness which shows itself in his impatience of useless knowledge and his interest in defending a sphere for the exercise of faith. But his real place is with the intuitionists of the Platonising school, with the men of enthusiasm and of vision, believers in an Absolute to be felt and seen, rather than to be thought. In this respect he is the forerunner of Jacobi and the faith philosophers with their opposition of *Vernunft* and *Verstand* and their rejection of demonstration. To Pascal, too, we can recognise his debt in spite of the contrast in spirit. There is the same distrust of reason in matters of faith, the same sense of the necessity of practical choice in the absence of knowledge, the same reliance on the reasons of the heart—but here the likeness ends. Pascal is a Christian, Rousseau, in spite of his profession, is not. For the former, the only escape from the ignorance and misery of life is by the *salto mortale* of faith in the supernatural revelation of God. Even this faith itself is a gift of God to a human nature too radically evil to will its own salvation. For the latter, truth is to be had by a return to the natural innocence of the heart. For Rousseau there is no tragedy, no agony, no great gulf fixed between fallen man and his salvation, his destiny is in his own hands. In Pascal's classification it is with the Stoics that Rousseau would belong. With Kant his relation is more largely one of stimulus than of logical likeness. It was to his preaching of anti-intellectualism in morals and religion that Kant recognised himself as indebted.¹ But, while they are at one in their belief that these aspects of life have a foundation other than that of science, they differ in their conception of what it is. Instead of the postulates of the moral life, Rousseau relies upon the primitive instincts of human nature, upon an intuition which, while not the *intellectuelle Anschauung* which Kant rejects, is, in its immediacy, liker to the *Vernunft* of the faith philosophers than to the practical reason of Kant. In this respect he is not so far along the path to pragmatism as is his more illustrious successor.

¹ Hartenstein, ed., viii. 618, 642; cf. V. Delbos, *Rousseau et Kant*, *Revue de métaphysique et de morale*, 1912, also his *La phil. pratique de Kant*, ch. ii.

III.—MR. RUSSELL'S LOWELL LECTURES.

BY PROF. L. P. SAUNDERS.

A CRITICISM.

I SHALL here attempt to show that Mr. Russell's most recent account of our knowledge of the external world is, on purely general grounds, of little or no philosophical value.

As, then, the objections to be raised refer to what may be described as his general standpoint, treatment and presuppositions, I shall not have much to say about the details of his position. These have already been subjected to criticism by Mr. Pritchard and others.¹

For the sake of brevity, I shall assume that Mr. Russell's Lowell Lectures have been read. This should reduce the insertion of quotations to a minimum.

In my opinion, Mr. Russell's whole account is vitiated partly because he introduces distinctions that do not exist and leaves out others, of great philosophical importance, that do; and partly because his purely 'logical' method of solving difficulties is epistemologically unsound.

But, like everything Mr. Russell writes, this book is astonishingly clever, ingenious and suggestive—his "manipulations" are quite amazing!

An hypothesis that is not purely fictitious presupposes facts to be explained; and these facts must, of course, be distinguished from the facts of the hypothesis, which may be called, in contrast to the others, the assumed facts. If an hypothesis is the only one that explains the facts, and if the facts are not existentially independent, then it is true. And this may be taken to imply that the assumed facts form part of the same "universe of discourse" as the presupposed facts. So much, I think, would be admitted.

It is also generally held that of rival hypotheses that one is the *best* which is the least complicated, other things being equal. But as the '*best*' hypothesis is not necessarily the true one, this distinction cannot claim any logical support.

¹ Notably Mrs. Adrian Stephen's extraordinarily clear and brilliant discussion.

It cannot be proved that the least complex among hypotheses is the true one; and it is certainly not self-evident that it must be. That is to say, there is no logical justification for preferring a simple explanation to a complex one. For, after all, there is no reason to suppose that Reality has a bias in favour of 'short cuts'.

The facts presupposed by an hypothesis, those which, in other words, it claims to explain by assuming certain *other* facts, stand in need of amplification. They are not *mere* facts, in the sense in which, for instance, sense-data may be said to be; but facts known or thought, truly or falsely, to be related in definite ways. That this is so is obvious; for, otherwise, there would not be anything to explain. Similarly, the facts of the hypothesis, the assumed facts, are not *mere* facts.

In truth, a *mere* fact is *not* a fact at all. So long, therefore, as this is recognised, it is enough to say that an hypothesis assumes facts to explain other facts. And this is just why *mere* facts, entities like sense-data in their purity, cannot as such either explain anything or be explained by anything.

Further, all hypotheses are conceptual constructions; but not all conceptual constructions are hypotheses. Conceptual constructions that do not explain anything are not hypotheses.

If a 'thing' is conceived as related in a way in which it is not known to be relatable or conceived to have properties not known to be compatible with its nature, the concept may be called ideal; if it is conceived in a way in which it is known not to be relatable, the concept may be called fictitious; and if it is conceived in a way in which it is relatable it may be called real.

A metaphysical explanation claims, as such, to be true; a scientific explanation, as such, only claims to explain. Hence ideal and fictitious concepts may enter into scientific, but not into metaphysical explanations. In other words, if an explanation is metaphysical, the concepts it uses must be real. This, of course, is not to be taken to mean that Philosophy, any more than Science, should proceed independently of hypotheses. Whether it should or not, I do not here claim to decide. The point is that if Philosophy proceeds hypothetically, its hypotheses must at least claim to involve real concepts. Thus, if an hypothesis is either avowedly not of this sort, or can be shown not to be, it cannot claim any metaphysical significance.

Logical manipulation is a process that has as its products

logical constructions. A logical construction is, so far as I can see, a group or collection constituted by known entities conceived as related in a way such that they, as a whole, shall have or necessarily involve certain (assigned) properties —stated otherwise, a logical construction is a group or collection of known entities conceived as related in such a manner that it, the group or collection, shall have certain (assigned) properties.

The entities that enter into a logical construction may be called the constitutive entities, the relations may be called the constitutive relations, and the 'involved' properties, the assigned properties.

Thus we may say that logical manipulation is the process that 'converts' known entities into constitutive entities, and properties into assigned properties; and it does this by conceptually relating known entities in a way that (necessarily) 'involves' their having the properties assigned.

Whether or not the assigned properties are the properties of something else does not affect the character of the process, manipulation, as such, nor the nature of its products, logical constructions.¹

However, a construction may be called a translated construction when its assigned properties are 'borrowed,' *i.e.* the properties of something else, real or presumptive.²

The following definitions are also relevant to the discussion, *viz.* :—

A construction may be called abstract when its constitutive entities are pure abstractions, *i.e.* are either (i) sense data, or (ii) entities of the same order.

A construction may be called concrete when its constitutive entities are concrete, *i.e.* not pure abstractions or entities of the same order as sense data.

A construction may be called ideal when its constitutive relations are unverified, in the sense of not known to be, relations of entities of the kind of its constitutive entities.

A construction may be called real when its constitutive relations are verified (*i.e.* known to be) relations of entities of the kind of its constitutive entities. A construction may be called a fictional construction when its constitutive relations are known not to be relations of its constitutive entities.

One of the peculiarities of logical constructions is the way in which they are arrived at. You start with certain properties, and the problem is to 'define' other things in such

¹ Hereinafter by construction I shall mean logical construction.

² Whether or not all assigned properties are borrowed, I do not so far decide.

a way that they, as an interrelated whole, shall have or involve these properties—*i.e.* it may be said to consist in defining a ‘subject’ that shall as such involve certain predetermined properties.

It must be admitted that there is no logical difficulty in conceiving two different kinds of thing to have some identical properties in common. But it is impossible to conceive either (1) instances of different kinds having the same properties, or (2) instances of one kind having no common properties. Hence if a *fact* x has as its properties α, β, γ , a construction y cannot have as its constitutive properties these very properties; but it may have either only some of them or all of them and more besides. This is, I suppose, quite clear.

Next, it is important to realise that, as Hume pointed out long ago, it is not possible to “infer” to know *a priori*, the properties of anything. The question is therefore raised as to what can be meant by saying that things related in certain ways as such “involve” certain properties. It seems quite clear that all “reasoning” is hypothetical, since it *assumes* universality or identity. Hence when it is said that the relatedness of certain things in certain ways “involves” their having certain properties, all that can be meant by “involves” is either that the things in question, related in the given ways, are *known* to have the properties said to be “involved” or are apprehended (*i.e.* “reasoned”) to have them on the assumption of their identity to other things *known* to have them. (It is in this sense that reason is “indirect knowledge”.)

These considerations show that *all* constructions are hypothetical, in at least the broad sense that they are not known *facts*. Hence if Mr. Russell claims anything at all for his special hypotheses, he must show that they are, in some important sense, better than those hitherto formulated. I hope to show that he has completely failed to do so.—To continue:

Propositions involving concepts that are either ideal or fictional may be said to contain ‘uncertain’ factors or ‘matter’. Such propositions may be called impure. A proposition that involves no ‘uncertain’ factors (*i.e.* a proposition that only contains real concepts) may be called pure.

An impure proposition is said to be logically translated when, its meaning remaining unaltered, real concepts are substituted for its ideal or fictional concepts. The products of logical translation may be called translated propositions. An untranslated proposition, therefore, will be an impure

proposition that has not been subjected to logical translation.

It is clear, I suppose, that if logical translation has any metaphysical value it must claim either (1) to constitute as such a proof of the truth of the propositions translated, or claim (2) that its products, translated propositions as such, are more probably true than untranslated propositions.

A logical objection to logical translation would contend either (1) that a proposition cannot be logically translated without altering its meaning, or (2) that if a proposition can be translated without any alteration in its meaning, it is inconsistent to maintain either (i) that the translated proposition is true and the untranslated proposition false, or (ii) that the degree of probable truth of the proposition untranslated is less than that of the translated proposition. Now although neither of these objections, in themselves, go any way to showing that the 'new' propositions supposedly reached by 'translation,' are not true as such or not more probably true than the 'original' impure propositions, none the less they are conclusive, if valid, against the possibility of logical translation. I submit that the logical objections in question are obviously valid. If so, logical translation must be regarded as the process that substitutes real constructions for ideal or fictional concepts. Thus understood, a proposition X may be said to be the logical translation of some other proposition Y, if the constructions contained in X are real *and* constructions whose assigned properties are the *same* as those of the ideal or fictional concepts contained in Y. It follows from this that the constructions contained in a translated proposition are translated constructions (see p. 31). This does *not* imply that the concepts of *all* pure propositions are translated constructions.

If some propositions involve more than one concept or construction, there can be degrees of purity, and so, of course, degrees of translation. From this point of view, a completely translated proposition would be a pure proposition.

I now go on to state, in my own words, what I regard Mr. Russell's main thesis to be and what I think it implies:—

I. In the degree to which (scientific) propositions can be purged of uncertain 'matter' to that degree will their probable truth be increased. In other words, pure propositions are more probably true than propositions that are not pure or are less pure.

II. This elimination can be effected by logically translating

(scientific) propositions in 'terms of abstract constructions' (see p. 31).

III. A complete translation of *all* scientific propositions by means of abstract constructions would be either the truth about, or what is most probably true of, Reality in so far as it is *judged*.

I believe this is a fair statement of Mr. Russell's general position and its implications. Still, I wish to emphasise the fact that Mr. Russell himself does not make the precise statements that I have given as essentially representing his contentions. It is not improbable, of course, that I have, through misunderstanding, misinterpreted them. I hope, and believe, I have not. But should I have done so, then a good deal of what I shall say will be irrelevant as criticism of Mr. Russell. And then my only excuse will be that the points raised and discussed are of importance in themselves. However, if I am doubly deluded, Mr. Russell, I feel sure, will make this amply clear—should he think it worth while.

If one of the following propositions can be substantiated, Mr. Russell's position (or what I take his position to be) must be rejected, *viz.* :—

a. That scientific propositions cannot be logically translated into propositions whose constituent concepts are abstract constructions.

β. That if the elimination of uncertain 'matter' could be effected by logical manipulation and translation, this in itself would neither constitute (i) a proof of the truth of the resulting body of translated propositions, nor (ii) add to the degree of their probable truth.

I am of opinion that both of these contentions can be shown to be valid. I shall consider objection *β* first.

β (i) If it can be shown that the grounds for asserting the truth of a pure proposition must be other than the fact of its purity, it will have been shown that the logical translation of an impure proposition, howsoever it be effected, goes no way in itself to establishing the truth of the translated proposition.

To do this it is only necessary to point out (1) that the truth of a proposition is other than its purity; (2) that impure propositions may be true; and (3) that pure propositions may be false.

If calling a judgment pure means that it is verifiable, then, of course, every pure judgment is true, since to be verifiable is to be true. The plain fact is, however, that the words pure and verifiable are not synonyms; and to suppose that they are, is to regard logical translation as equivalent to

verification, or, at least, to a process that *makes* propositions verifiable. Now, as a matter of fact, it is nothing of the sort, nor, it should be clear, does it claim to be. Logical translation can only, and does only, claim to substitute real constructions for concepts that are not real. To contend, therefore, that pure judgments as such are true is to contend that what *may* be true *is* true. And this is untenable.

In brief, logical translation is not verification. It is also important to realise that verification is not judgmental.

The above is a general argument against maintaining that translated propositions as such are true. But, so far as Mr. Russell's general standpoint is concerned, at least two other special arguments may be adduced. For Mr. Russell, uncertain 'things' are unverified 'things,' *i.e.* 'things' that are not *given*. Thus, admitting that the *truth* of a proposition is other than its *purity* (and I do not see how this can be denied) Mr. Russell can only hold that pure propositions are (certainly) true if he is prepared to maintain that their truth is *given*—or, at least, that the *truth*¹ of the general proposition *All pure propositions are true is given*. Further if it could be shown that Mr. Russell's translations are *not* logical translations, on the ground that *his* constructions are not real, the fact, if it were one, that translated propositions are true would go no way to showing that the propositions he submits are true. I shall attempt to show further on (see p. 40) that his constructions *are* either fictions, or ideal constructions.

This concludes my substantiation of the first part of objection β . Before proceeding to the second part, I wish to emphasise the fact that the sense in which I am using the word verifiable is both the only important sense and also the sense in which Mr. Russell uses it. Throughout verification means for him being *given* (in intuition). Thus, in speaking about the notion of cause, he says, "In so far as a causal law is directly verifiable, the thing inferred and the thing from which it is inferred must both be data, though they need not both be data at the same time. . . . But we cannot become acquainted with a particular except by its being actually given. Hence the particular inferred by a causal law must be only *described* with more or less exactness; it cannot be *named* until the inference is verified"²—*i.e.* until the inferred particular is *given*.

¹ Incidentally, I should like to point out that to speak of some propositions as being *self-evidently* true is nonsense. The truth of a judgment cannot be self-evident (this statement is an analytic definition), and no universal judgment can be verified, *i.e.* *given*.

² Pp. 213, 214.

In any other important sense of verification, untranslated propositions are just as much or as little verifiable as translated propositions, impure propositions as much or as little verifiable as pure propositions. And, so far as their truth is concerned, my contention is, that, even in the strict sense of verification, a pure proposition as such is not more verifiable than an untranslated proposition. The only propositions that are verifiable are true propositions.

β (ii) I have now to show that the elimination of uncertain matter effected by logical translation does not even add to the degree of *probable* truth of the resulting propositions. In other words, I contend that the degree of probable truth of a translated proposition cannot be asserted to be higher than that of its 'corresponding' untranslated proposition—or, what comes to the same thing, that higher degrees of purity do not as such involve higher degrees of probable truth. I also shall attempt to show that, even if it is a fact that the degree of probable truth of a pure proposition is higher than that of an impure proposition, the degree of probable truth of Mr. Russell's 'translated' propositions cannot be maintained to be higher than that of untranslated propositions.

I shall, adduce then two arguments, one a general objection, the other a special objection.

The general objection will have been substantiated if it can be shown either that (1) there is no sense in saying that the degree of probable truth of *any* proposition is higher than that of any *other* proposition; or that, whether this be a fact or not, (2) it is not legitimate to maintain that the degree of probable truth of a translated proposition is higher than that of the corresponding untranslated proposition. And to establish the special objection it is only necessary to show (3) that Mr. Russell's constructions are not real.

To this end, a brief discussion of probability is necessary. And this I approach indirectly. Suppose a bag contains five balls, four red and one white. According to the mathematical theory of probability, the chance of drawing a white ball is $\frac{1}{5}$, and of drawing a red $\frac{4}{5}$. Precisely what these statements mean is a disputed question. But I think all would agree that fractions representing chances or degrees of probability cannot be regarded as applying to *particular* events. If they stand for anything at all, it is of the proportionate order. That is to say, what is affirmed in all such cases is, stated generally, that in an infinite number of occurrences of

the event in question the proportion of its occurrence in some specified way to some other specified way is such and such a proportion. Whether or not this proportion can be determined exactly is, I understand, a disputed point. Thus, in the example given, it may be asserted *either* that in the infinite number of events that is constituted by an infinite series of drawings the proportion of white to red balls drawn is (or would be?) $\frac{1}{2}$, or that the proportion is some fraction that falls short, or exceeds, $\frac{1}{2}$ by an infinitely small amount. Of the two, I prefer the first. But both, it seems to me, are entirely gratuitous; and by this I mean that there is no evidence in favour of either, *i.e.* if they are put forward at all, they must be put forward as *a priori* judgments or ultimate belief. This is a point, however, that I shall not consider. And I am not sufficiently versed in the mathematics of infinite numbers to be able to decide whether their 'nature' is such that it is possible to assert, in the case of an infinite number of events, that the proportion of events of one kind to events of another kind is such and such a proportion.

What I do wish to urge is that if numbers representing probabilities are interpreted in some such way as the one just given that then the theory of probability is intrinsically inapplicable to the *truth* of propositions, if it is a fact that a judgment is either true or false, and not possibly sometimes true and sometimes false. Admitting this it cannot be argued that the chance or probability that a judgment X is true (or false) is $\frac{1}{2}$, since this would mean that \bar{X} , "in the long run" (an infinite 'run'), was as often true as false. In saying that this cannot be maintained all I mean is that it is inconsistent with the view that a judgment cannot be sometimes true and sometimes not true. I do not intend to assert that, in itself, the position is either nonsensical or untenable. And, fortunately, I need not attempt to decide the question. For, if what is true (or false) cannot be sometimes not true (or not false), then no meaning can be attached to the statement that a proposition P is more probably true than some other proposition Q; and if what is true (or false) can sometimes be not true (or not false), then all that could be meant by saying that a proposition P is more probably true than some other proposition Q, is that P is *always* true and Q only sometimes true or never true. Hence in maintaining that translated or pure propositions are more probably true than untranslated or impure propositions either nothing at all is asserted or else what is asserted is that the former are *always* true and the latter either are only sometimes true or

are never true.¹ But I submit that this is not what is intended; and, in any case, it seems quite unjustifiable.

On the other hand, if the numbers representing probabilities do apply, directly or indirectly, to *particular* things or events, it seems plausible to argue that the probabilities, whatever they may be, representing the different possible ways of occurrence of an event X also represent the degrees of probable truth of the propositions that severally assert X's occurrence in those ways. Thus, if the probability that a white ball is drawn in any given draw is $\frac{1}{5}$, and that of a red $\frac{4}{5}$, it seems plausible to maintain that the degree of probable truth of the proposition, *A white ball was (or will be) drawn*, is $\frac{1}{5}$, and that of the proposition, *A red ball was (or will be) drawn* is $\frac{4}{5}$.

But this cannot be accepted because, as I have already pointed out, a particular² proposition is simply either true or false, i.e. there are only two alternatives.

This seems to be one way of showing that numbers representing chances or probabilities do not apply or refer to particular events; since the only meaning, it is plausible to contend, that can be attached to a statement to the effect that the chance of drawing a red ball at some particular drawing is, say, $\frac{4}{5}$, is to regard it as an indirect way of asserting that the chance that the proposition is true that affirms the drawing of a red ball at the particular drawing in question is $\frac{4}{5}$.³

Consider another case. If A and C stand for propositions, A^1 and C^1 for their contradictories, then one or other of the following conjunctives is true, viz., AC^1 , AC^1 , A^1C , AC^1 . Hence it is plausible to maintain that the chance that the conjunctive AC is true is $\frac{1}{4}$; the chance that the alternative *Either AC or AC^1* is true $\frac{2}{4}$; that of the alternative *Either AC or AC^1 or A^1C* $\frac{3}{4}$; and that of the proposition *Either AC or AC^1 or A^1C or A^1C^1* , $\frac{4}{4}$. Here again, however,

¹ Or, as another alternative, that the former (pure or translated propositions) are sometimes true and sometimes false and the latter never true, or that the proportion of times true to times false is greater in the one case than in the other.

² By a *particular* proposition I here mean an *instance* of a class of propositions that are all 'about' or 'of' the same fact.

³ It is important to realise that assertions that express chances are not either 'problematic' or 'apodictic,' but 'assertoric'; and it is also important to realise that they claim to be true, and not to be "probably" true—whatever this may mean. That is to say, propositions that express calculated 'chances' claim to be true propositions. In other words, all such calculations start with facts (or supposed facts), and 'deduce,' validly or invalidly, other facts (or supposed facts). Succinctly put, what is probable is a fact, and not, so to speak, a favoured possibility.

the contentions must be rejected, and for the reasons already given. In the present case the apparent cogency of the claims made is due, I think, to confusing the truth of beliefs with beliefs as such. That is to say, there may be some sense in saying that the chance or probability that any *one* of the four conjunctives is *believed* is $\frac{1}{4}$; but none in saying that the probability that any *one* of them is *true* is $\frac{1}{4}$.

I conclude that it is meaningless to speak of any proposition as being more probably true than any other proposition.¹

This conclusion may be reached from a different point of view. What is asserted is always either a predicate or a relation, and a predicate or relation either *is* or is *not* the predicate or relation of that of which it is asserted. Otherwise put, *all* propositions are "assertoric". A "problematic" proposition is an elliptical hypothetical—² e.g. S *may* be P, means that if S is M it is P, i.e. SM *is* P. And a so-called "apodictic" proposition is not a *kind* of proposition, but, if anything, a kind of predicate or relation. But, my point is, to assert that S is *probably* P either means that the probability of S being P is such and such, or else it has no meaning at all.

This concludes the first part of the general objection (see p. 36 above). I have now to show that even if it is legitimate to maintain that some propositions are more probably true than others, it is not legitimate to maintain that translated propositions are more probably true than the corresponding untranslated propositions.

Let TP stand for the class of translated propositions, and UP for the class of corresponding untranslated propositions; also let $tp_1, tp_2, tp_3 \dots$ etc., etc., stand for the members of class TP, and $up_1, up_2, up_3 \dots$ etc., etc., stand for the corresponding members of class UP. The position in dispute affirms that tp_1 is more probably true than up_1 , tp_2 more probably true than up_2 , etc., etc. Now as a 'thing' S_1 can only be affirmed to be more probable than some other 'thing' S_2 if the probability of the former is higher or greater than that of the latter (this is at least *one* essential pre-condition) it follows that the probability that tp_1 is *true* is greater than the

¹ By saying that X is more probably true than Y, I mean that the probability that X is true is higher than that of Y. I do not want to be told that I am confusing probability with probable; for part of what I contend is that the meaning of probable (or more probable) is either the meaning of probability (or higher probability) or else it has no meaning at all. This part of my contention is clearly brought out in the brief discussion given above.

² Not in the way in which a particular proposition is an 'elliptical' universal, for a particular can only be said to 'imply' a universal, and not to be a universal.

probability that up_1 is true, that that of tp_2 is greater than that of up_2 , etc., etc., if the members of class TP are severally more probably true than the corresponding members of class UP. But, unfortunately, there is no way of establishing this 'fact'. To calculate the probability of anything 'being something in particular' it is necessary to establish first of all an alternative judgment claiming to set forth all the relevant mutually exclusive and collectively exhaustive particulars. My contention is that in the case in question no such relevant alternative judgment can be made.¹

I now proceed to substantiate the special objection referred to above (p. 36)—that Mr. Russell's 'translated' propositions are not more probably true than untranslated (scientific) propositions because his constructions are not real but are either ideal or fictional (see p. 31)—i.e. on the ground that Mr. Russell's 'translations' are not, *ex hyp.*, translations at all.

According to Mr. Russell, whatever is *given* is certain; and among *given* entities he includes all sense data, some 'facts' of introspection, some of memory, and a few simple and directly apprehended relations. These certain entities may be called *data*. Besides data, there are other entities, not factual but judgmental, which he also regards as certain, *viz.*, some purely "logical" propositions. These may be called certain *premises*. And, as we have seen, his contention is that propositions involving constructions whose con-

¹ Against the general arguments adduced in the text it may be urged that Mr. Russell simply means there is *more* reason for believing (i.e. asserting) that translated propositions are true than there is for believing that untranslated propositions are true. Now if this does not mean that the probable truth of the former is greater, or if it does not indirectly imply this, then my answer simply is as follows: A proposition is either true or false, and a proposition can only (logically) be asserted to be true if it is implied in some other true proposition or other true propositions. This means, in effect, that there either is or is not *reason* for asserting a proposition's truth, and that, so far as logic is concerned, there is no such thing as *more* reason—the latter, if it has any meaning, is psychological.

Surely, it may be argued, if the fact asserted by a proposition P explains *all* the facts of a given order, there is *more* reason for believing P than *some* other proposition Q, that only explains some of the facts of the order in question? This is a mistake. There could only be *more* reason for believing P and less reason for believing Q in the sense that there was *some* reason for asserting P's truth and none for asserting Q's. But the "same reason" for asserting P's truth must be, logically, not a reason, but *the* reason. Generally what that reason is I have already stated. In the present case, P could only be asserted to be true, if it could be shown that the fact asserted by it was the only fact that explained the given order of facts, and then only if the facts in question are facts and facts which are not existentially independent.

stitutive factors are certain—*i.e.* are *data*—are more probably true than propositions involving constructions that are not of this order—with the exception, apparently, of those purely logical propositions which he accepts as indubitable, *i.e.* with the exception of certain *premisses*.

Now if it can be shown that Mr. Russell's constructions are not real, it will follow that his translated propositions cannot as such claim to be more probably true than untranslated propositions. To this end I shall proceed to examine Mr. Russell's claim that the integral parts or constitutive factors of his constructions are "verifiable" entities—*i.e.* certain or indubitable. This examination will be comparatively lengthy, and involve the discussion of points that are not directly relevant to the issue in dispute. I introduce them here on the grounds of convenience.

I begin with a brief account of expectation, belief, doubt, inference, and certainty, and their relations.

A belief is an *asserted* content. To believe R is to *assert* R. If assertion and judgment are synonyms, then *a judgment*, in the sense of an instance of assertion as such, is not a belief. Otherwise *a judgment* and *a belief* are synonymous terms. To *infer* R is either to know or to assert R—*i.e.* an inference, in the sense of a *conclusion*, is either a belief or something known. This may be objected to on the ground that what is inferred is not as such believed or known—*i.e.* on the ground that a conclusion as such is not belief or knowledge. But this objection is due, I think, to confusion. I admit that R, in the syllogism, *e.g.*, P, Q ∴ R, is not necessarily believed or known. This, however, is irrelevant, since R, I contend, is not the *conclusion*, but '*R is implied by P and Q*'. This is the real *conclusion* or *inference*, and this is either believed or known. Further, it is important to realise that *implication* is not the only relation assertable. But when this is the relation *asserted* or *known* (*i.e.* immediately apprehended) it may be convenient to call the judgment, belief or knowledge an *inference*. Similarly, you may call inferring *disimplication*, if by this is meant that what is asserted or known is a relation of implication. From this point of view, every judgment or belief is not an *inference*. But every inference is either a judgment (or belief) or an instance of knowledge.

Finally, in this connexion, it is misleading to speak of *inferring* a belief or judgment. Such a statement is meaningless. And I think it is very important to realise this. A belief or judgment is an asserted 'content,' is 'something' asserted, and to say that, *e.g.*, the assertion of R is *inferred*,

either means that what is asserted is a relation of implication, or else it has *no* meaning at all. To repeat, my contention is that to say, *e.g.*, that R, a judgment, is *inferred*, can only mean that R is an asserted implication, and *not* that the *assertion* of the implication R, whatever it may be, is *inferred*. In a word, to *infer* is to *assert*; to say, then, that a belief is inferred comes to saying that an asserted something is an asserted asserted something; and this is pure tautology.

But just as there is no sense in saying that the 'origination' of any belief is *inferential*, similarly there is no sense in saying that it is *causal*. And it is here that Mr. Russell¹ falls into error, and is involved in inconsistency. His position, I think, may be fairly expressed as follows: Beliefs are either ultimate or non-ultimate, and non-ultimate beliefs are either *inferred* or *caused*. A non-ultimate belief may be said to have 'grounds'; the 'grounds' of inferred beliefs are *premises*, those of caused beliefs are *facts*; the relation between an inferred belief and its grounds (*premises*) is logical; that of a caused belief and its 'grounds' (*facts*) is extra-logical. To avoid the implication that ultimate beliefs arise *in vacuo*, or through some sort of spontaneous generation, the position, which I take to be Mr. Russell's, had better be restated in the following manner: The 'grounds' of beliefs are either logical or extra-logical; logical grounds are *premises*, extra-logical grounds are not *premises*. A belief whose grounds are *premises* may be called an *inference* or an inferred belief; a belief whose grounds are not *premises* may be called a non-inferred belief. The relation between an "inferred" belief and its grounds (*premises*) is that of "implication". These are his kinds of non-inferred beliefs, the grounds of one of these kinds are *facts*, and the relation obtaining between such non-inferred beliefs and their grounds is that of *causality*; the grounds of the other kind of non-inferred beliefs (*e.g.* "logical" propositions) are not *facts*, and the relation obtaining between them and their grounds is neither causality nor implication. The latter kind of non-inferred beliefs may be called *ultimate*.

This is what Mr. Russell says: "Psychologically, a belief may be called derivative whenever it is *caused*² by one or more other beliefs, or by some *fact of sense*³ which is not simply what the belief asserts. . . . If we call a belief "logically primitive," when it is not actually arrived at by a logical

¹ A mistake that is committed, it may be mentioned, by most psychologists.

² Italics mine.

³ *Ibid.*

inference, then innumerable beliefs are logically primitive which psychologically are derivative. The separation of these two kinds of primitiveness is vitally important to our present discussion."¹

From what has been said it should be evident that I am of opinion that this position is radically unsound. Granted that it is not nonsense (and I think it is) to speak of *inferred* beliefs, there is no doubt at all, I think, that it is meaningless to speak of beliefs being *caused*. Such a position involves a fundamental misconception of mental 'phenomena'. For in whatever sense the origin of *any* beliefs is "logical" or "inferential," in that sense the origin of *all* beliefs is "logical". If a "psychological" account of a belief is significant at all, it can only claim to state what in fact were the "grounds" of that belief. The point I make is that these grounds, for the believer, always or never claim to be "logical," no matter what the belief is. In a word, if *any* belief is "inferred," *all* are—*i.e.* *every* belief is, in the same sense, an "inference". I do not say that they are all "legitimate," or that the "grounds" of all beliefs are reproducible to the mind of the believer, much less, then, that they are always 'before' the mind of the believer.

Further, I deny that there are any ultimate *beliefs*. The truth of a belief as such cannot be self-evident. A *fact* may be 'immediately' apprehended; but such apprehension is not *belief*; it is *knowledge*.

Finally, Mr. Russell's division of beliefs into *caused* and *inferred* is inconsistent with his own account of *causal laws* and *causes*. "A causal law," he says (p. 213), "allows us to infer the existence of one *thing* (or *event*) from the existence of one or more others. The word 'thing' here is to be understood as only applying to particulars, *i.e.* as . . . including sense-data, with whatever is logically of the same type as sense-data." And further on (p. 220) he says: "The word 'cause,' in the scientific account of the world, belongs only to the early stages, in which small preliminary, approximate generalisations are being ascertained with a view to subsequent larger and more invariable laws. We may say, 'Arsenic causes death,' so long as we are ignorant of the precise process by which the result is brought about. But in a sufficiently advanced science, the word 'cause' will not occur in any statement of invariable laws. There is, however, a somewhat rough and loose use of the word 'cause' which may be preserved. The approximate uniformities which lead to its pre-scientific employment may turn out to

¹P. 69.

be true in all but very rare and exceptional circumstances, perhaps in all circumstances that actually occur. In such cases it is convenient to be able to speak of the antecedent event as the 'cause' and the subsequent event as the 'effect'. In this sense, provided it is realised that the sequence is not necessary and may have exceptions, it is still possible to employ the words 'cause' and 'effect'. It is in this sense, and in this sense only, that we shall intend the words when we speak of one particular event 'causing' another particular event. . . ." These statements are not, I think, altogether unambiguous; but it at least seems clear that for Mr. Russell "causes" and "effects" are "particulars," *i.e.* either sense-data or whatever is of the same order as sense-data. As, therefore, a *belief* is not, in this sense, a "particular," it is unquestionable, I think, that Mr. Russell's contention that some beliefs are psychologically derivative is inconsistent with his own account of causality.

I now shall briefly consider doubt, expectation, and certainty. Doubt is neither mere ignorance nor mere supposition, it is, I think, denial; it is not, however, denial of the truth of a judgment, but of the conclusiveness of evidence put forward in support of a judgment. In other words, to doubt R is not to *disbelieve* or to *deny* R, nor is to assert or believe R's evidence to be inconclusive; it is simply either the non-acceptance, on the part of the doubter, of R's evidence as evidence or is an analytic statement whose import is that R's evidence is not evidence of R. In the latter, and proper sense, denial is knowledge, and not belief.

Expectation is a special kind of belief, a belief in some *future* occurrence; and as no belief is caused, mere repetition cannot be the cause of expectation. Repetition may cause revival; but revival is not belief.¹ This is why Hume's account (and Mr. Russell's so far as he follows Hume) is inadequate. Revival (if there is any such thing)

¹ Mr. Russell says, "Derivative beliefs . . . constantly arise . . . merely by association of ideas . . ." (p. 69). Now as "association" only gives rise, directly, to "revived" ideas and images, the contention made literally interpreted is that revived ideas and images as such *cause* beliefs. This is absurd, and not, I think, what Mr. Russell means. He may hold that the mere recurrence as such (*i.e.* without knowledge of its recurrence) of an idea causes belief in *that* idea. But this is not the view he has here before his mind; his position is that the ideas revived through association cause beliefs in "ideas" *other* than the "revived" ideas—*e.g.* the idea of eating in certain circumstances comes before, is "revived" in, the dog's mind, and Mr. Russell's position is that this "revived" idea *causes* the dog to have two beliefs (1) that he ate then (?), (2) that he will eat now. Are these opinions of Mr. Russell's "soft" or "hard"?

is not as such recollection ; it is, in fact, quite hypothetical, and, so far as I can see, an hypothesis that has never been unambiguously described, nor one, besides, that clearly explains anything it is claimed to explain. Further, *what* is remembered is *not* what is expected—*e.g.* the child's knowledge of the fact of having eaten under circumstances that it recognises as similar to its present ones is *not* the fact it expects. I say “knowledge,” because recollection *is* knowledge. What the relation of facts remembered is to other facts is another question. Here again Mr. Russell, I think, falls into error ; for he regards memories as beliefs—some as “certain” beliefs. But no one *believes* what is remembered ; although what is remembered may be believed to be related in some way to other facts. This, however, is another matter. I submit that Mr. Russell confuses them with each other.¹

Next what is meant by a “certain” belief, a “certain” fact, or a fact of the highest degree of “certainty”? It is clear, I think, that what is meant by a “certain fact” (or by a “fact of the highest degree of certainty”) is either a “certain” belief (or a belief of the highest degree of “certainty”) or simply *knowledge* of a fact ; since it is quite inappropriate to speak of certain *facts* or of certain *knowledge*. Now what is to be understood by certainty as a *qualification* of belief? My answer is that it has no meaning whatsoever, *unless* there is such a thing as degree of belief. This view is held by some philosophers (*e.g.* by Prof. J. S. Mackenzie) but what is meant is very far from clear.²

¹ It may be objected that what Mr. Russell *says* is that some facts of memory have the highest degree of certainty (p. 72), and *not* that some memories are certain beliefs. My reply is that neither *beliefs*, facts nor knowledge can be certain. See discussion of “certainty” that follows.

² Pragmatists might maintain that to assert a concept is preparedness to act upon it *as if it were a fact*. And, from this point of view, a “certain” belief would be a concept (*i.e.* a conceived fact) that was *invariably* treated as if it were true (*i.e.* representative of a fact) and so *always* acted upon. All other “beliefs” (*i.e.* all beliefs other than certain ones) being “asserted” provisionally and in default of better ones, in the sense that they are *used* or acted upon, but not *used* without a consciousness of risk. This may be stated more clearly. To believe *is* to assert, and to assert *is* either acting upon, or preparedness to act upon, when the opportunity arises, a conceived fact *as if it were a fact*. And from this point of view a belief or assertion may be said to be “certain” if the conceived fact acted upon without any consciousness of risk, and uncertain when there is a consciousness of risk—or, in order to avoid the imputation of interpreting belief in terms of belief (on the ground that consciousness of risk as here understood is belief), it may be maintained that degrees of “certainty” are, at bottom, degrees of utility. But how far such a position is adequate and inconsistent I do not propose to consider.

I submit that there is no such thing as varying degrees of belief. The exigencies of a situation may demand decision and action under circumstances in which the means adopted for the fulfilment of our purposes may be of doubtful efficacy; but this does not mean that we have a low degree of belief in the relevance of our undertaking. The contrary supposition involves confusion, and gives rise to the view that there are degrees of belief. The confusion consists, I think, in identifying, *e.g.*, *R doubted* with *R "doubtfully asserted*, and then, confusedly, treating the *R* as if it were asserted with a low degree of belief, thereby overlooking the fact that doubt is either purely negative or else equivalent to denial, and so (in the latter case) is *knowledge* and not *belief* at all.

I contend, in brief, that certainty is not a qualification either of *facts* or *knowledge* or *belief*. Certainty and degrees of certainty, if they stand for anything at all, are predicates —*i.e.* something assertable, and not intrinsic properties of assertion as such.

I think that all these considerations go to show that Mr. Russell's division of "data" into "hard" and "soft" is untenable—beliefs as such are neither certain nor uncertain, and no belief is "psychologically derivative". All that Mr. Russell ought to mean when he says that *some* "data" are "soft" is that some beliefs are not *known* to be true. But then this is the case with *every* belief.¹ And this fact I think is, curiously enough, at the "back" of Mr. Russell's mind, and so really at the foundation of his division of data into "hard" and "soft". For otherwise what are his real grounds for holding that psychologically derivative "facts" (beliefs) are dubitable or uncertain and psychologically primitive "facts" (beliefs) certain or indubitable? Surely at least part of what he means by this is that some "facts" may be false and others (the psychologically primitive ones) are certain or indubitable in the sense, in the end, that they are *known*. And in that case his division should be into "facts" that are not known and *facts* that are known, *i.e.* into *belief* and *knowledge*. But if this is not his meaning, then all I have to say is that he has in no way justified his contention that beliefs that are caused (*i.e.* psychologically derivative "facts") are any less certain, in any important sense of certainty, than beliefs that are either not caused (*i.e.* psychologically primitive "facts" and *a priori* ultimate beliefs)²

¹ If *R* is believed, it ceases to be believed when it is verified, *i.e.* known.

² The relation between *a priori* "judgments" ultimate beliefs, and psychologically primitive beliefs is not clear. Where an *a priori* "judg-

or beliefs for which some "argument"¹ is adducible, *i.e.* "inferred" beliefs. For if a psychologically primitive "fact" is not a fact *known*, it is either a non-caused *belief* or an "inferred," *i.e.* "logically derivative," belief; hence in neither case is the fact believed known, *i.e.* in both cases the beliefs are "uncertain". And in what important sense has Mr. Russell shown that beliefs that are either not caused or beliefs that are "inferred" are less uncertain than caused beliefs? The fact is, I think, Mr. Russell has not seriously considered what an "inference" is. I have already pointed out that an "inference" is either a judgment or an analytic statement of an immediately apprehended relation of implication—*i.e.* it is either belief in or knowledge of something; in the former case the "evidence" is not transcended, in the latter case, as in all judgment or belief, it is. "Arguments," as ordinarily understood, cannot "prove," *i.e.* verify, a judgment or belief.

But if Mr. Russell does mean by being certain being known, then half of what he says is irrelevant and practically the whole of the remainder inconsistent—*i.e.* as far as his general position is concerned—and most of it is confusing.

Consider his meagre list of "certain" or "hard" data. These are (1) the "Laws of Logic," (2) sense-data, and whatever is of the same type or order, *i.e.* whatever is *given*, *viz.*: (a) certain simple temporal and spatial relations, also relations of similarity and difference, (b) cases of motion falling within the specious present, (c) some introspective facts, and (3) some facts of memory. I examine them in order—(1) Mr. Russell does not give a list of the "Laws" he has in mind. But whatever they are they must either be *believed* or known. If believed they are not certain; and on any other important sense of certainty, the "Laws of Logic" are no more certain than a great many other beliefs.

(2) Sense-data can only be certain in the sense of being known. There is no meaning in saying that sense-data as

ment" is not a mere supposition or hypothesis, it is an ultimate belief. A "psychologically primitive belief" is not a *belief* at all, or else it is a belief but not *primitive*, if by a primitive belief is meant one whose content is *given* and in no part asserted or conceived. It is only because what is "psychologically primitive" is not *belief*, but *knowledge*, that it is significant to contend that it does not stand in need, in contrast to what is "psychologically derivative," of justification. Further, an ultimate belief is not to be confounded with a so-called self-evident belief, when of the latter is meant a belief the evidence of whose truth falls within its own content, or rather is its own content. A "self-evident belief" is not a belief at all—in fact, the expression is self-contradictory.

¹ See p. 74.

such are *believed*; thus to talk of beliefs in sense-data as being certain or highly certain is simply nonsense. As such, sense-data are meaningless, *i.e.* are not facts, in the sense of being complex entities. It is clear therefore that they are not *in themselves* judged. And if only facts are known, it also appears that they, sense-data as such, are not known. But may be Mr. Russell simply means that their *existence* is "certain". If this is so, I submit that this can only mean that their existence is *known*. And in that case what is known is something complex. On any other interpretation, I must regard the following statement as entirely devoid of meaning, *viz.*, "The more we reflect upon these [*i.e.* sense-data and the general truths of logic], the more we realise exactly *what they are* [italics mine], and exactly what a doubt concerning them really means, the more luminously certain do they become" [p. 71]. Mr. Russell, if he is saying anything at all, goes too far when he says that reflection upon sense-data makes us realise exactly *what they are*. If Mr. Russell knows exactly what they are, it is not only a pity he has not enlightened us, but it is also a pity he should have wasted his time in devising useless and superfluous logical "constructions" to explain them.

(a) Only in so far as instances of similarity and difference are *known*, are these relations "certain". This is also the case with "certain" temporal relations. But it cannot be admitted that *any* spatial relations are "certain". I do not deny their "certainty" on the ground that spatial relations as they are may be different from what they *appear* to be; but because to *know* them, the point of view must also be known. As, however, Mr. Russell does contend that relations are not always what they appear to be, it is clearly inconsistent on his part to hold that any relations are "certain".

(b) Again if any motions are "certain," this must mean that some motions are *known*.

(c) Mr. Russell does not give a list of the introspective facts that he regards as certain. But it is fair to suppose that the facts he has in mind are those of "inner" sense, *viz.*, feelings such as pain, organic sensation, images, etc., etc., and *in some sense* (?) all desires, emotions, instincts, beliefs, ideas, aims, etc., etc. Mr. Russell, of course, does not trouble to explain (indeed, I think he has not bothered to find out) precisely in what sense any of these things are "certain". To reply that he simply means that they are *real* is clearly inadequate, until he explains what he means by being real.

(3) I have already pointed out that recollection or memory is not *belief* in but *knowledge* of. Mr. Russell is inconsistent, then, when he says that only some memories are certain. But if in saying that some memories are certain he means that some beliefs about some memories, *i.e.* remembered things, are certain, then, from his general standpoint, this claim is gratuitous and, from mine, it is untenable, for no belief is certain.

From this brief survey it appears that Mr. Russell's "hard data" should claim to be facts *known* and not facts *believed*. It would seem, further, that Mr. Russell is not aware of this, and that, whether he is or not, he is either not clear as to what these facts are *or else* he does not clearly define them. And, finally, he does not see that a great many other facts, excluded by him, are just as "certain," in any important sense, as his "hard data". So that, once again, there seems to be inconsistency.

Mr. Russell, I have to point out, regards the laws of logic and sense-data as the hardest of hard facts; thus, in the end, the other hard facts (*viz.* (a), (b), (c) and (3) above) are not really "certain". It is fair, therefore, to say that the only facts that are known, according to Mr. Russell, are sense-data and the laws of logic; everything else is, at bottom, either conjecture or supposition or belief. I submit, however, that his laws of logic are not known to apply. Now this is, in point of fact, his own view; for Mr. Russell has told us elsewhere (*Problems of Philosophy*) that every *a priori* proposition is hypothetical. He is left, therefore, with sense-data.

Now if anything of any philosophical value could be done with sense-data, Mr. Russell, above anyone else, would do it. But if anything is clear, it is clear that sense-data are not sufficient, are not adequate as foundation for any sort of superstructure that can claim any metaphysical importance. It is, I admit, a little late in the day to have to point this out; and quite astonishing that Mr. Russell should lay himself open to this sort of criticism.

Mr. Russell, like all empiricists, does not take his own position with sufficient seriousness. He tells you, in effect, that sense-data *alone* are certain facts; and in violent contradiction to this he asks you to accept a great many other statements (*viz.*, the statements constituting his position as such, statements about it, and statements about other philosophies) as true! And yet one would have thought it unnecessary to have to point out that if sense-data are the *only* really certain (*i.e.* certain) facts, that then nothing else is

certain. Unfortunately, although this is, in one sense, quite clear, it is also, in another sense, not clear, seeing that this statement itself claims to be true. How Mr. Russell came to overlook this is very difficult to understand.

Mr. Russell is quite right, in one sense, when he says that the more we reflect upon sense-data the more illuminating do they become. This is not, however, because, as he thinks, they are sense-data, but because reflection is illuminating. What more, in general, can Kant's Criticism claim than this? Is Philosophy anything more than "Criticism," *i.e.* anything more than illuminating reflection?¹

To revert. It should be clear now why all Mr. Russell's constructions are abstract, and it should also be clear that they are all ideal. They are abstract because their constituent entities are sense-data, and ideal because their constituent relations are not in themselves "certain," or "certainly" relations of entities of the kind that enter into his constructions. And this is why Mr. Russell's translated propositions are not really translations at all [see p. 40].

This concludes my substantiation of objection *β*. I now proceed to substantiate objection *α*, *viz.*, That scientific propositions cannot be logically translated into propositions whose constituent concepts are abstract (translated) constructions [see pp. 34 and 40]. This, it is clear, is a general contention. And it will have been established if it can be shown that an abstract construction is either an ideal or fictional construction. Against Mr. Russell, and by way of a special objection, it is only necessary to show that *his* constructions are ideal. This I have already done.

All that is presupposed in the elaboration, or definition of a construction is, stated generally, that certain things in certain relations have certain properties which otherwise they would not have. This, in itself, is incontrovertible—*e.g.* lines related in certain ways may be said to constitute

¹ Philosophy, I may be allowed to say, is not a way of persuading ourselves or others that nothing, or next to nothing, is known; it is, if anything at all, an eye-opener, so to speak. That is to say, Philosophy deepens, and not darkens, our minds. And this is the case even if it is essentially disillusionment. Surely, Mr. Russell agrees with me? Really, we *know* a very great deal without Philosophy, and with it, properly understood, we 'discover' much more. It may be heresy, but even so I suggest that the proportion of facts *known* to facts *believed* is not by any means unfavourable to the former—indeed, I submit, that we know *more*, perhaps far more, than we believe. Anyway, if nothing else, this is a salutary protest. So much "Philosophy" seems to be little more than a heroic attempt to *speak* consistently; or else it consists in just abstracting all meaning from things, followed by a grave attempt to get most of it back "logically," *i.e.* surreptitiously.

figures of different orders having specific properties. However, a *triangle*, e.g., is not a "construction"; for its constitutive entities (lines¹) are related in the way that involves their having, as related, the specific properties of such a figure; whereas the constitutive entities of a "construction" are not related in the conceived way, but, in the case of a "real construction," *relatable* in that way—i.e., a *triangle* is not a "construction" because its constitutive 'parts' (lines) are not merely relatable in the given way, but related in that way. In brief, a "construction" as such only claims to be *possible*. Strictly, then, "ideal" and "fictional" constructions are not "constructions" at all. So that if a concept is a "construction" it must be "real". And from this it should be quite clear that "translation," if it has any value at all, must claim to substitute possible concepts for concepts that are either impossible or not known to be possible. Further, if the *meaning* of the translated proposition is different from the meaning of the original (or untranslated) proposition, it must be "proved" on independent grounds—i.e., its "evidence" cannot be the "evidence" of the untranslated proposition—unless it can be shown that the "evidence" of the untranslated proposition only justified that part of its meaning that is the meaning of the translated proposition. And, after all, this does constitute an independent "proof" of the translated proposition. But, be it noted, either *all* propositions that are subjected to translation are purely assertions about relations of properties or some are not; if they are, then they can only be translated into propositions that assert something other than a relation between properties; if they are not (if, that is, some propositions that are translated assert something other than a relation between properties) then, again, the propositions into which they are translated must assert something else. In either case, therefore, the "proof" of the translated proposition must be independent of that of the untranslated proposition. All this may be stated concisely as follows: A translated proposition either asserts what its corresponding untranslated proposition asserts or it does not; in the former case there has been no translation; in the latter case there has been, but then the evidence of the translated proposition must be independent of that of the untranslated proposition in the sense, at least, that their "evidence" cannot be identical. And this difference cannot be the fact of translation, since translation is not as such evidence of the truth of the translated proposition.

¹ In the end, there is no reason why the *lines* rather than the *angles* should be regarded as the constitutive entities.

To return to the main point. Abstract constructions are ideal because, for one reason, it seems true that the only ways in which sense-data are *known* to be related are by *similarity*, *difference* (perhaps quantitative as well as qualitative in both cases) and time, ways that do not seem to be "evidence" of, or to "inferentially" involve, any other properties—anyway, not when, as with Mr. Russell, metaphysics is "analysis". Another reason is that only *things* have properties, and not pure abstractions like sense-data, no matter how they be conceived as related—*e.g.*, the *whole* constituted by *lines* related in a given way is a *figure*, and as such it involves certain properties, properties that constitute its nature or make it the sort of figure it is. In other words, *only* in so far as being related in a particular way constitutes the *relata* or *terms* into *parts* of a *thing* can being related *involve properties*. Briefly, only *intrinsic* relations "carry with them" or "involve" properties—a relation is intrinsic when it is *part* of a *thing*; and *every* relation is intrinsic to some *thing*, but not to every *thing*. This consideration is, I think, of general importance, apart from its relevance in the present discussion. Only, therefore, when the "constitutive relations" are intrinsic to the "constitutive entities" (*e.g.* sense-data in the case of abstract constructions) of a "construction" can the former "involve" the latter having the "assigned properties". But in that case the defined whole is not a "construction," it is, on the contrary, a *thing*; for to say that the "constitutive relations" are intrinsic to the "constitutive entities" means that they *are* both *parts* of one and the same *thing*.

This concludes my substantiation of objection α , and therefore, as objection β has already been discussed, my criticism of what I take Mr. Russell's main thesis to be. There are a good many other contentions made by Mr. Russell that I should like to discuss, but I must refrain from doing so for want of space.

IV.—SPECULATIONS ON THE WORKING OF THE BRAIN.

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PHILOSOPHY presents to the experimental physiologist a curious problem. When in a strange and clumsy manner he enquires of its attitude toward the problems with which it deals he finds that there is by no means unanimity in the explanations offered. He finds, indeed, that Professor X. takes a materialistic view in the philosophic explanation—the “why”—of a series of phenomena upon which Mr. A. looks from the standpoint of the idealist. The physiologist knows that differences of opinion exist in his own subject. He thinks that these may readily occur where the data are insufficient—that they then serve as a stimulus for the accumulation of new facts and, having played their part, disappear. But philosophy (he supposes) must have its data as nearly complete as possible, and another explanation of these differences of opinion must be forthcoming. Again he knows that the influence of schools, and even personal vanity, may sway the thought of men so that criticism is blurred and some data are too much emphasised and other data too little. But in his own subject, where fortunately the burthen of schools has been a light one, he has experienced the invariable failure of bias. Therefore he hesitates to attribute all the differences of philosophy to such causes.

The problem now begins to interest him from his own standpoint. Professor X. and Mr. A. find different explanations for the same series of data. He cannot question their sincerity. Is it possible that there may exist two kinds of truth? The conclusion to which he comes—a tentative conclusion, for the problem cannot be attacked in his own manner—is that the one thinker is born to take one attitude and the other another. Their brains differ in structure, there is a corresponding difference in the functioning of the brains, and the concomitant mental phenomena are different.

In such cases there are admittedly differences in mental attitude between different individuals even where they are presented with apparently identical series of data. But each of us knows that his mental attitude may differ with regard to a specific problem at different times. Take, for example, so crude a case as "belief in ghosts". At certain times, for instance in the broad light of day, we say with conviction that the evidence brought forward with regard to their existence is insufficient—in short, that we "do not believe in them". But which one of us has not, upon some night when seated alone reading in a room, experienced the sudden feeling that all the powers of darkness were behind the chair? At such times, when it requires courage to turn the head, not all the strength of our reason will drive away the dreadful something that lurks in the shadows. Then we must believe although logically we cannot believe. At one and the same time we believe and yet do not believe. Are there two kinds of ways in which the brain works, or in which its working is conditioned?

This question at once brings us to the consideration of the problems encountered in the physiology of the central nervous system—the problems of the manner in which it works. But before attempting to trace the development and present position of this subject let me draw your attention to a point of great importance. The experimental study of the physiology of the nervous system is a purely objective science. We have nothing to do with the subjective phenomena. Consciousness and sensation accompany many if not all of the phenomena which we investigate, but our province is the examination of these phenomena from the material and physical standpoint. We must admit, however, that this restriction of the aspect from which we look upon the subject—a restriction of comparatively recent appearance—has its drawbacks, although upon the whole it is beneficial for the advance of knowledge. The psychologist and the physiologist look at different sides of the same problem. Neither can use the data of the other—the psychologist because he cannot interfere experimentally in the manner of the physiologist, and the physiologist because where he interferes experimentally he has no test for the subjective phenomena. As it were, each is blinded of an eye and the seen object is flat. Will it ever be possible to see with two eyes the object in its fullness, to perform the final synthesis? That question lies in the future.

The physiology of the nervous system arose from vague beginnings—as did all the sciences. It was observed that

animals reacted to outside influences. The reaction might be similar to that which in man accompanies the sensation of pleasure, or to that which accompanies pain—and so on. The lower animal was therefore endowed in the thought of men with a sentient principle—the “*sensorium commune*”—just as man himself was. The seat of this was guessed to be in the brain and spinal marrow.

Again it was observed that (for instance) when the skin of an animal was stimulated a reaction indicative of pain occurred. It was guessed that this was due to a stirring up of the sensorium commune through the agency of the peripheral nerves. Perhaps this was no rude guess but a deduction from the observation that after injury to the nerves in some cases the irritation of parts of the skin in man is not felt.

At this time it was held that the movement of an animal was an index of its consciousness, of the activity of the sensorium commune. It was thought that certain nerves carried a flow of the animal spirits into the central parts, that the sensorium commune was there disturbed, and that then occurred a flow of the animal spirits in the reverse direction either in the same nerves or in other nerves. When the animal spirits arrived at the muscles they caused them to contract.

In some such manner as this the conception of “reflexion” arose. Descartes is usually credited with the invention of this sense of the word, but this meaning of the word is not given in Castelli’s admirable Lexicon (1762). Robert Whytt in the middle of the eighteenth century uses the more usual term “sympathy”. He would say that, if a thorn pierces the skin of the foot and that limb is moved, a sympathy exists between the skin of the foot and the muscles of the limb. This sympathy is conditioned by the activity of in-going and out-going nerves which have a common bond in the central sensorium commune. As we would say to-day—there is a reflex path between these two parts of the body.

Now two problems were presented by this conception of the working of the nervous system—the problem of the seat of the sensorium and that of the location of the point of reflexion.

With regard to the question of the point of reflexion the older observers relied upon the anatomical evidence alone. Suppose again that a thorn pierces the skin of the foot and the limb thereupon moves, it is at anyrate possible that there occurs a direct stimulation of the muscles of the foot. Robert Whytt deduced, from the fact that muscles far removed from the seat of irritation may take part in the re-

action, that the "sympathy" between the parts of the limb is not so direct—but that it occurs through the nerves.

When the nerves are examined anatomically it is found that if they are followed up towards the spinal cord they become collected together and gathered up into ever greater bundles and that near the spinal cord they form great networks or anastomoses. It is clear that the seat of reflexion might be here and not in the spinal cord itself.

There grew up to be two schools in connexion with this problem. One said that the nerves run through the networks and come into intimate relation with each other only in the spinal cord itself. The other said that there occurred an intimate connexion of the in-going and out-going nerves in the networks outside the spinal cord. This question was settled by the experiments of Stephen Hales and Robert Whytt who showed that the phenomenon of reflexion disappears after the destruction of the spinal cord. It is now known that the in-going nerves come into close contact with the cell-bodies or the processes of the cell-bodies of the out-going nerves in the grey matter of the spinal cord as well as in other parts of the central nervous system. The actual functional distinction between in-going and out-going nerves—between sensory and motor nerves—was in part established by Sir Charles Bell, but the chief credit of this generalisation is due to Magendie.

As it were, the seat of reflexion and the sensorium commune were thus placed in the central nervous system. Even before its localisation in this region the question of the unity of the sensorium had arisen. This problem shaped itself somewhat as follows: Consciousness and reflexion are functions of the sensorium commune: is that sensorium an indivisible unit; and is it spread as it were all over the whole central nervous system, or may it be more minutely localised?

As is well known, Descartes localised the soul in the pineal gland. But the sensorium appears to have been considered as distinct from the soul—for instance, it is present in the lower animals—and it was generally supposed to be in a manner connected with the whole nervous system. Francesco Redi and Robert Boyle, however, showed that when cold-blooded animals are decapitated they may remain alive for days and exhibit reflex movements of their trunks and limbs. These experiments demonstrated that the sensorium commune was not located in the brain alone. Gilbert Blane showed that after decapitation the phenomenon of reflexion may be present both in the head and in the trunk, and he

showed also that if then the spinal cord be cut across the phenomenon may be present both in the upper and in the lower segments of the trunk. If the reflex phenomenon is associated with a sensorium commune that must therefore be capable of sub-division; and as a matter of fact Blane drew the conclusion that the automatic functions of the nervous system may take place without the intervention of the sensorium commune—without the presence of consciousness and sensation. There persisted, however, echoes of the unsolved and perhaps unsolvable problem whether or not the activities of the spinal cord are accompanied by consciousness right into the middle of last century when, under the influence of the new thought introduced by the Darwinian theory, the subject was finally abandoned as without the province of experimental science.

The localisation of the seat of reflexion was a more legitimate point for investigation than the question of the consciousness of the spinal cord; and it was found that comparatively small portions of the spinal cord may suffice for the exhibition of certain reflexes. Legallois, for instance, showed that the phenomenon of breathing—which had by that time been included amongst the automatic or reflex acts—stops when a comparatively small part of the grey matter of the upper part of the spinal cord (*medulla oblongata*) is destroyed. Similar restricted localisations were made for other reflex phenomena.

Gradually the conception of the reflex became crystallised in its modern form. It is recognised that the in-going nerve carries a state of disturbance from its end in the skin towards the central nervous system. There it has another ending in close connexion with some part of an out-going nerve. The disturbance can pass between these two nerves and run in the out-going nerve fibre down to a muscle or some other part of the body the activity of which it may affect. The in-going nerve and the out-going nerve form what is known as the “reflex arc,” and when they work together in this manner they exhibit the reflex phenomenon.

Of course this conception is a very artificial one. There may be, and probably are, almost always more than two nerves in the arc. But it is not necessary for us here to do more than refer to this possibility.

Now this conception of the “reflex” has been used as the functional unit in speculations on the manner in which the nervous system works; and a great body of experiment has been directed to the investigation of the reflex itself. Sherrington, who has done more to increase our knowledge of

the working of the nervous system than any other living or dead investigator, has shown us how the central nervous system binds reflex with reflex and integrates the activities of the different parts of the body.

We have reached by this time such a knowledge of the properties of the reflex that we may ask the question: how we are to look upon the greatest and most complex activities of the body such as the activity of walking or progression?

At first we are inclined to deny that walking is a reflex or automatic act, and to think of it as a complex movement slowly and painfully learnt. But this is probably an incorrect attitude to take up. It is said that certain animals—such as the young chamois—are able to run almost immediately after birth; and it is also said that some birds are able to fly immediately on coming out of the egg. Recent experimental evidence has proved that the young mammal can in fact perform some of the movements of progression under conditions in which any possibility of "learning" the movements is out of the question. We must in fact admit that progression is really an automatic and reflex act, just as breathing is, although it may be to a certain extent under the control of the will.

Now if progression is really an automatic or reflex act how is it performed? The usual explanation is that it is brought about by an ordered sequence of different kinds of reflexes. The movements of the limbs and of the trunk during progression were long ago carefully studied by means of photographs taken in rapid succession during the act. As we all know, two of the chief movements of a limb in walking are those of bending and stretching—flexion and extension. When we walk, one limb is drawn up towards the body and carried forward while the other limb is in contact with the ground. While this first limb is still in the air it is again extended until it touches the ground. Shortly after it comes into contact with the ground the other limb in its turn is drawn up to the body—and so the cycle proceeds.

Until a comparatively recent date no very convincing analysis of the conditions under which these movements occur was attempted. Although the movements as such were examined it seems that it was generally supposed that they were conditioned by the action of the will, and the question was left at that. For a peculiar reflex movement which resembles progression, however, Freusberg as early as the commencement of the last quarter of the nineteenth century attempted an explanation which is prophetic of the modern views, but the subject was not followed up. Philippson at

the commencement of the present century was the first to give a more complete explanation.

Sherrington's classic investigations amongst many other things had established a general law of reflex movements—namely that when the skin of a limb is irritated that same limb is flexed and the opposite limb of the pair is extended. These movements—the “flexion-reflex” and the “extension-reflex”—are similar to the movements which occur in progression. Philippson therefore suggested that progression is composed of an alternate sequence of these reflexes. With this idea he analysed the photographic records of progression movements in terms of simple reflex movements. But the reflexes occur under the influence of irritation of the skin—“stimulation” as we term it—and the question of the conditioning stimuli for progression therefore arose. It is not possible here to go fully into the theory; but it may suffice to say that, in brief, Philippson thought that the contact of the foot upon the ground gave the effective stimulus to the skin. When one foot is pressed upon the ground it gives a stimulus which conditions the withdrawal or flexion of the limb towards the body. This movement removes the stimulus which conditions it, and the limb is again extended in a movement conditioned by the skin stimulus given to the other foot then in contact with the ground. So the rhythmic movements alternate automatically—each movement itself removing the stimulus which causes it.

Unfortunately this beautiful theory is not an adequate one. We must be quite clear about this. There can be no doubt that these skin stimuli play a part in the act of progression, but Sherrington has shown that they do not play the most important part. He demonstrated conclusively that progression may occur when the possibility of skin stimuli is excluded by experiment. His own position was similar to that of Philippson. Progression is due to the rhythmic combination of reflexes which are conditioned by stimuli self-abolished by the acts which they evoke. Where he differs is in the kind of stimuli which he supposes to condition the movements. We all know now that in the muscles there are internal sense organs which are stimulated by the movements of the muscles themselves. We speak of their activity in its psychological aspect as the “muscular sense”; and Sherrington showed that from the physiological standpoint these sense organs and their nerves act in a manner similar to the skin receptive organs. Stimulation of the incoming nerves from a muscle gives flexion of the same limb and extension of the other limb of the pair. He therefore

found in the organs which subserve the muscular sense the site of the stimuli which condition progression. Put simply, the contraction of a muscle which extends the limb gives the stimulus for flexion—in which that muscle relaxes and thus abolishes the conditioning stimulus.

Again there can be no doubt that these inner stimuli play a part in progression, but neither do these play the chief part—for the movements of progression may occur after the elimination of all peripheral stimuli. This raises afresh the whole problem of the conditioning of progression. Without doubt it is an automatic act, but it seems not to be a reflex act in the usual sense of the term.

The point of reflexion in the central nervous system has long been termed the "centre". At the centre there is usually situated a nerve-cell. An in-going nerve-fibre impinges upon this cell and one of the structural processes of the cell is an out-going nerve-fibre.

For many years now it has been recognised that in some cases the nerve-cell may be actuated by other means than through the in-going nerve. This is the case in the respiratory centre—that centre which governs the movements of breathing. There it is known that a change in the chemical constitution of the blood acts as a stimulus to the centre. That centre may, however, also be actuated through in-going nerves. We all know the gasp which follows a sudden douche of cold water.

With the failure of stimuli of in-going nerves to serve as the prime conditions of progression we must fall back upon a similar conception to that which we use in our theory of respiration. In other words, we must look upon the centre which governs the movements of the limbs in progression as acted upon both by changes in the chemical constitution of the blood and by disturbances carried through the in-going nerves. Indeed we know that in certain conditions changes in the blood may fire off these centres, and there are many other analogies between the movements of breathing and of progression. A most striking similarity is seen in their rhythm. In both of them movements of opposite sense alternate with regularity. The cells in these centres discharge rhythmically, and this rhythm seems to be conditioned in the centre itself. Theories have been put forward to account for the manner in which the rhythm is conditioned, but these are too complex to consider here.

The position may be maintained that progression is not a complex act slowly evolved by the adding together of unitary reflexes. It may rather be held that the rhythmic

movements are the original activities of the nervous system, and that the reflexes have as it were crystallised out of them by the appearance during the course of evolution of in-going nerves which can induce small parts of the original rhythmic phenomenon—as it were, its flexion side alone or its extension side alone.

In the light of this idea we must regard the motor (efferent) nerve-cell and its out-going nerve-fibre as the chief unit in the nervous system. It may be actuated in one of three ways. In the first place by the direct action of the blood; in the second place by the action of the in-going nerve; and in the third place by the action of nerves which descend from the brain and other higher parts of the central nervous system.

This brings us at last to the brain itself, but let us first clear up our conception of the centre. We speak of centres in the nervous system because we know that small parts of it may apparently function when separated off from the rest. This localisation of function has led many to a conception of the nervous system as composed of innumerable discrete centres linked together by nerve paths. As it were, the nervous system is looked upon as composed by the adding together of otherwise independent unitary centres. This conception may be a useful one, but it is probably more correct to try to think of these centres as all mutually dependent and functioning as a whole—*independent only when made so by experiment*.

In the case of the brain this conception of unity is a most important one. The brain is a great centre connected chiefly with the sense organs for sight, smell, taste, and hearing. In Sherrington's words, it is the head-ganglion for the exteroceptive receptors—or for the sense organs which receive their stimuli from outside the body. Certain parts of the great brain are more intimately connected with one or other of these sense organs than are others. If the back of the brain is destroyed blindness occurs, and so on. From these and similar observations it has been supposed that there is a localisation of function in different parts of the brain. When we "see" a thing a certain part of the brain is active. When we "hear" a sound a certain other part is active—and so on. But it is probably far more correct to say that the whole brain is active both in seeing and in hearing. Suppose, for instance, that it was possible to cut off the part of the brain usually termed the centre for vision from the remainder in such a manner that both parts were left alive within the skull. We can scarcely doubt that the

visual part would still "see," but the other part connected with the production of speech would tell us that it was blind. The chief part of the brain would be mutilated and could no longer act as a whole.

A centre has in-going and out-going nerves, and so too has the brain. Toward it there run fibres from the sense organs of the head, and in addition to these there are in-going fibres which carry up from the spinal cord the impulses which subserve the sensations of touch, skin-pain, and so on; and there are yet other in-going paths.

From the brain there run out-going nerve-fibres. Of the functions of some of these we know little or nothing, but the discovery of the function of one set of them was a chief influence in establishing the theory of cerebral localisation—the theory that different parts of the great brain have different functions. It was found that when a certain area of the surface of the great brain is stimulated electrically there may be produced various movements of the body. One small part of this area gives a movement of the fingers when it is stimulated; another part gives bending of the knee—and so on.

This so-called "motor area" is the seat in the great brain of large nerve cells which send down their nerve fibres into the spinal cord. There the fibres come into contact (directly or indirectly) with the motor cells of the cord. In other words, the motor cells of the brain act through the spinal centres; and at the same time it may be pointed out that the in-going nerves of the spinal centres send up branches to the brain—so that they act upon their out-going nerves both directly in the spinal cord and indirectly through the brain. The brain may indeed be looked upon as a great centre; but the thing which it acts upon directly is not a motor organ (or "effector") like a muscle but the spinal centre, so that it acts only indirectly upon the actual muscles by influencing the activity of the spinal centre.

Now with regard to this question of localisation of function. If we examine the spinal cord we find that certain parts of it are more directly concerned with certain reflexes than are others. The segments near the lower or tail end of the cord have chiefly to do with the movements of the lower limbs; those nearer the upper end have chiefly to do with the movements of the upper limbs—and so on. Because when, in experiment, we isolate these segments it is still possible to obtain reflexes which govern limb movements, we have come to look upon such reflexes as functional units. As we saw before, this idea of unit reflexes suggested that

the greater complexes are built up of their combinations. But this is probably an incorrect idea. Normally the nervous system acts as a whole, and there is no such thing as a reflex act confined to some small part of it. This is the case probably even where the reflex evoked is a weak one and where there are no obvious movements in the other limbs and parts of the body. It has been demonstrated experimentally that a reflex act which occurs in the case of one hind limb is accompanied by movements not only of the muscles of the other hind limb but also of those of the fore limbs and of the trunk, neck, and head.

When we are investigating the properties of a single so-called unit-reflex we are much in the position of the astronomer. We must remember that it is influenced by the activity of other reflexes. The astronomer can consider theoretically the relative movements of two known masses which revolve about each other free in space. But when he investigates the motion of a specific body in the solar system he knows that the system acts as a whole, and that the movements of any one body in it are affected by the position and movements of all the other bodies. Even more than this, he realises that probably these movements are also affected by the far-off stars, although his instruments are not sufficiently delicate to detect the influence of these bodies. In short, the astronomer is dealing with the parts of a complex unit—the solar system—just as we are in the case of the nervous system.

Let us assume, then, that the nervous system is a whole; that it contains centres and may be analysed by the investigation of unit-reflexes; but that these reflexes are as it were crystallised out of the common whole, and that the whole is not composed by the building together in a more or less fortuitous manner of discrete reflexes. The system is a whole. It contains within it many centres. And of these the brain is one of the most complex.

The nervous system in some manner is connected with the phenomenon of consciousness. This connexion raises many problems which are a matter at present for interesting speculation but scarcely for experimental investigation. One may be referred to here. Without assuming any such attitude as the acceptance of the location of consciousness in space we may yet ask whether consciousness is associated with the whole nervous system.

If we assume that the nervous system acts as a whole it may be argued perhaps that it is conscious as a whole. We have, however, no means of testing this—just as we have no

means of testing whether or not any other living person besides ourselves is conscious. In the latter case all we can do is to admit that "I myself" am conscious; to notice that other beings like myself react in the manners in which I react and relate experiences similar to my own; and to draw an inference that they also are conscious.

In a similar manner we may draw the inference that all nervous centres are in some manner connected with consciousness from the assumption that is forced upon us that consciousness is in some manner connected with some nervous centres. This inference may or may not be a correct one, but it is not one which can be shown to be incorrect. Take for instance the case of a man whose back is broken and yet lives—as many such cases do live. That part of the body below the broken portion of the spinal cord is regarded by us as unconscious. The upper part we believe from its behaviour to be conscious. But we have no right to deny consciousness to the lower part. All we can say is that it is deprived of all means of indicating to us that it is conscious.

Of one thing we may be sure, the consciousness of an isolated part of the nervous system is in some manner incomplete. In the above instance the upper part of the nervous system of the man is not so fully conscious as is the whole nervous system of a normal man. Many of his ordinary sensations are abolished. In a similar manner disease of a certain part of the back of the great brain cuts off the visual sensations, and the nervous system of the individual has no longer the complete consciousness of the normal man. He is blind without knowledge of his blindness. Just as the nervous system may be mutilated in such a manner as to destroy some of its reflex reactions so that it no longer acts upon its physiological side as a complete whole; so is it possible that in the same circumstances it no longer is completely conscious. But you must remember that all this is mere speculation, and as such perhaps illegitimate.

What then, from the physiological standpoint, is the function of this great centre which we call the brain? To put it briefly we may say that the action of the great brain seems to be one of control over the various movements of the body. Were the nervous system composed of the spinal cord and the little and middle brains alone its reactions would be mechanical, fatal, in a certain sense tragic. But the great brain appears to permute and combine the different components of these reactions into so many different forms that the activity of the whole system seems to lose its fatality and thus it appears to us to be almost un-predictable. The

system can react in an adequate manner to finer and more numerous circumstances and combinations of circumstances in the surroundings of the animal.

Now how are these permutations and combinations of reactions brought about? This is a question which we cannot at present answer, for our knowledge of the working of the brain is insufficient. But we can say something of the way in which the factors to be combined and permuted are presented to the brain.

The in-going nerve from the skin sets off a specific reflex reaction as regards the spinal cord, but at the same time it sends up impulses to the brain. We may say that these impulses act there upon a central mechanism which is originally of a certain structure and has previously reacted to many other different combinations of in-going nerve impulses. This mechanism then reacts and sets off its proper out-going nerves which conduct down to the spinal cord and in their action modify or replace the simpler reflex activity. But in its activity here the brain is acting as a reflex centre which is only more complex than the centres in the spinal cord. Its action is fatal, although its fates are not three in number but innumerable.

Now you see the point to which we have been advancing. It has been demonstrated that the spinal centre (or at any rate some spinal centres) are actuated not only by in-going nerves but also directly by the action of the changes in chemical constitution which take place in the blood. Some movements which seem thus to be conditioned may appear "spontaneously" when there is no apparent change in the external surroundings of the animal.

We look upon the great brain as a nervous centre. *May it too be actuated not only by in-going nerves but also by the direct action of changes in the blood?* There is much to be said in support of this view. We know that certain drugs which are carried in the blood to the brain may influence its activity. May not less gross blood changes play a normal part in the activity of the brain?

For myself I would say that this probably is the case and that changes in the activity of the brain (paralleled by changes in consciousness) are brought about by physical changes which may either be reflex or central in origin.

This idea introduces us to some speculations of interest.

We come back, for instance, to the problems with which we started. Two men may take radically different standpoints upon data apparently similar. But these data, while in themselves the same, may have different values to the

two—values which differ because the nervous systems differ in their workings and thus are differently affected by the same data. The nervous systems may differ in part organically; the two hypothetical philosophers are born with cerebral mechanisms of different kinds. In part the nervous systems may also differ because they have been subjected to different educations—that is to say that they have been subjected to different reflex influences. On such lines some of the differences in thought between individuals may perhaps be explained.

But it is within our own individual experiences that certain stimuli may at different times give us different kinds of thought. Think of the wide star-lit sky of a dark and cloudless night. The visual images which we receive may call up thoughts of the formation of the universe which we know; the distribution of the stars in space; their different distances from us. Or they may call up other thoughts and we may delight ourselves with the tracing out of the constellations, then to pass to the remembrance of the old legends with which their names are associated. Or we may have quite other thoughts and feelings, the feeling of the beauty of it all. Here external stimuli which are similar and act upon a mechanism fundamentally the same may yet at different times affect our consciousness in different manners—perhaps because of small differences in its state of activity at these different times.

And yet again, certain stimuli—visual images—may at first convey to us a clear meaning which on further analysis seems to be elusive. Take for instance the expression "Will to Power". That seems to convey a very clear meaning which is hard to come by when the sense of the individual words is examined. Or again we read in the paper to-day "The Belgians have crowned the world's admiration" [*Sunday Observer*, 18th Oct., 1914]. The meaning of this is quite clear, but the wording is obscure. We say that the writer has not clearly expressed the thought which he wishes to convey. But the thing of interest is that when first we read the sentence the meaning is clear. It is only in a later analysis of the words that obscurity is seen. And if we turn to certain poets this sort of clearness masked in obscurity is often encountered. Many better instances could probably be found, but at the moment there occur to me George Meredith's lines :—

Master the blood, nor read by chills,
 Earth admonishes : Hast thou ploughed,
 Sown, reaped, harvested grain for the mills,
 Thou hast the light over shadow of cloud.
 Steadily eyeing, before that wail
 Animal-infant, thy mind began,
 Momently nearer me : should sight fail,
 Plod in the track of the husbandman.

GEORGE MEREDITH, *A Reading of Earth.*

Here the meaning leaps at you and appears to transcend the words in which it is framed.

In such instances the written word—the external stimulus—has an effect upon our consciousness which appears not to be mediated through the reflex mechanism—the mechanism of logic. Can it be that our consciousness is affected in two manners ; by emotion and by logic ; poetry and science ?

And here we return to those curious conflicts of knowing which we all experience. Our knowledge at times of the powers of darkness is as strong as our knowledge at other times that these do not exist. Even within a short period of time we may experience these two antagonistic states of knowing so that we may almost say that at one and the same time we know that a thing does and that it does not exist. Is it not easiest to assume for such phenomena that the effective mechanism of the brain can be actuated in two different manners—from two different sides ?

And letting our minds run still further afield in this line of thought, what about the phenomenon of memory ? Consider for a moment. You may be following this argument or you may have allowed your mind to wander off in some fresh direction. In neither case is it possible for you to recall an event which for the moment has no connexion with the matter with which you are occupied. Let me give you some little stimulus—the word “*field*”—and as it were a new chain of thought is started and you may recall events or places or persons which you may think that you have long forgot. From the functional standpoint it is hard to suppose other than that the chain of thought is paralleled by an ordered series of functional (that is, physico-chemical) changes which follow upon the primary stimulus and are what they are as a resultant of the factors which have conditioned previous activities of the same material mechanisms. In this case the chain is set agoing by the little redistribution of energy on the part of the sound waves which are associated with the saying of the word “*field*”. On the starting of the chain may not the inner stimuli which play in the brain (as we have suggested) keep it up ?

But a curious phenomenon of memory is its apparent "spontaneous" origin in some cases. Before we fall into sleep at night there is often a wild riot of thought, and events long forgotten may refresh themselves in our consciousness. In such circumstances the ordinary external stimuli of the working day are in large measure excluded, and our hypothetical internal stimulation mediated through the changes in chemical constitution of the blood may well play a more important part in the activation of the brain. They have the field to themselves. It may be suggested that such spontaneous memories may be paralleled by functional changes conditioned by the inner stimuli which may play upon the brain.

Poetry and logic! Central stimulation and reflex stimulation! Is not life a continual struggle between the two? The child forgets the glorious palaces and the princess of the crystal mountain when he goes to school and is there drilled in thought—drilled reflexly. But he makes other dreams and creates other heroines although this dreadful prison of logic and convention may keep them chained within his own kingdom. And later, when we are conquered by the orderly processes of logical thought upon which our living depends, haply we may still dream dreams; and the least fortunate of us may get his dreams from others, reflexly it is true and through the written word—but dreams none the less.

If it be held that thought upon the one hand may be ordered more or less reflexly by a drilled logical and formal mechanism while upon the other hand it may be conditioned more "spontaneously" (that is, by central or inner stimulation) in a mechanism less inexorably knit together it might be supposed that in the experimental sciences the former method held sway while the latter was left to the poet.

This seems to me, however, not to be the case.

When we write a paper in which the results of experiment are described it is written perhaps in part to tidy up the loose ends of the line of research at which we are working. But chiefly it is written to explain to others (we pretend) the data which we have obtained and the conclusions at which we have arrived. The written words of our paper can influence others only through the reflex mechanism and the logical side of their minds. Therefore we marshal our facts and our experimental results in a logical sequence. "Having found that (so-and-so)" "it next became necessary to examine (this or that)"—and so on. In much of our research the order of experiment is that actually followed in

the writing of the paper. But in some cases the later fact is first as it were jumped at and the preceding facts then found and filled in. Yet when the paper is written the more logical order of arranging the facts is used. This might be considered an insincere and illegitimate proceeding, but it is not so. It must be remembered that the experiments are done for the pleasure of the investigator, for the satisfaction of his curiosity. But the results are written for the satisfaction of the reasoning of others.

This logical form in description, which is as it were in some cases historically untrue, tends to make us think that in the case of others the research is performed in the same logical sequence and that the final result follows naturally and inevitably from the preliminary results. In some cases this is of course what actually does take place in the course of the research itself. The research then has a sort of reflex nature. Perhaps the original problem may even be suggested by another. The line of research is seen to be spread out. The final result may almost be predicted. In such instances the whole thing is mechanical. It calls for little but technical originality. Much research is done in this way.

But the great results seem often to arise in another manner. Which of us has not experienced the sudden "idea" in surroundings perhaps most incongruous? At the billiard table, on the golf links, when reading a novel, the idea suddenly springs at us. Does it work? This must be tested, and the experiments are performed—finally to be presented in the historically inaccurate but necessary logical order so that the conclusion seems to follow from the line of research and to be conditioned by it. Darwin's work is surely a classical example of this. In such cases the central mechanism has, it is true, been constantly disciplined by logical thought—reflexly. But suddenly the central factors condition the new combination, the new hypothesis; as it were spontaneously.

Then there occurs the war of the two conditions of central functioning. The dream is disciplined by the logic, and logic wins the day in the final presentation.

It must not, however, be supposed that, even if this idea of the central actuation of the nervous mechanism be so important as has been here assumed, there is a strict demarcation between the two manners of central stimulation. Rather it had better be assumed that in all central activity there are the two factors, but that sometimes one and sometimes the other is the stronger.

When we look out upon the nations we may seem to find

indications of the predominance of one form of thought or the other in this nation or in that. In Germany there are many fine and original minds, but very much of the research done is mechanical—the research of the drill sergeant. In France there is perhaps upon the whole too little of the discipline of experiment, but thought effervesces. It is unnecessary, and perhaps at present unwise, to follow such speculations further.

And now at last to the end. I have tried here to let my thought travel unfettered, and have said much of rashness in this riot of speculation. Will you forgive me for its inexactness, the only excuse is the satisfaction of curiosity in following out a line of thought. Whether or not the position can really be held I cannot at present judge.

But in conclusion, and for the sake of consistency, it may be said that if this speculation has some truth in it there are thoughts above logic—thoughts to be clothed only in the language of imagination. And so, following this right to the end, it may perhaps express the idea if we say that the functional activity of the brain which you investigate from another side is to be looked upon not as a clear mountain stream, complex yet mechanical in its conditions down from the hard rocks of the parent mountain to the nirvana of the sea. Rather as a pool in an enchanted wood blown upon and rippled by the winds of heaven and so far understandable, but ever and anon stirred from the depths as if some mythical beast turned in its sleep and sent up iridescent bubbles to compete with the ripples. And the dragon a blood-dragon which we must strive to capture for its secret, a secret which may give us the fairy wish.

V.—DISCUSSIONS.

THE PHILOSOPHICAL BASIS OF THE *A FORTIORI*.

IN the inference

A is greater than B, B is greater than C, therefore A is greater than C . . . (1)

"we see directly," affirms Dr. Mercier, "that the conclusion is valid,"¹ and (again directly) that in other inferences superficially of the same type, analogous conclusions are not valid—his point being that the validity and the invalidity alike are in all such cases directly seen.

Now the soundness of this contention cannot be tested until we determine what is meant here by "directly". If, e.g., I were to say that increased illumination causes pupil contraction directly, Dr. Mercier would reply that while that might be true for the layman, the physiologist must regard the effect as indirectly brought about through nervous process—as indirect, i.e., compared with the "direct" action of light on a camera plate or on selenium.² Similarly in (1), what appears to be at first sight a direct perception of a logical necessity is, for the psychologist and the logician, in reality an indirect process; the truth being that what we "see directly" forms, once early infancy is outgrown, an extremely minute element of experience. Certainly no inferred conclusion truly such—no "therefore"—can ever be seen "directly"—"i.e., from the premisses as they are stated, without praying in aid a principium from outside"; and in the instance cited, Dr. Mercier himself shows this to be the case; for we find (p. 85) underlying the *a fortiori* the two general principles of substitution and implication—with (more remotely) the purpose of the argument; but none of these are within the premisses taken strictly in themselves.

This important qualification of his original contention appears more plainly in Dr. Mercier's contrasted instance (p. 89) of cheating. Here the premisses "give no warrant for substituting" and

¹ MIND, January, 1916, p. 83.

² It is of course a question whether we really ever have absolutely direct physical effects—the progress of science depending largely on the resolution of reactions apparently direct, into indirect; and the causal regression being in theory infinite, no limit can be placed in advance upon the number of possible intermediate changes; cf. Dr. Bosanquet, MIND, October, 1915, p. 97.

"reveal no relation between A and C"; the obvious implication being that in (1) they do (taken strictly in themselves) give such a warrant and reveal such required relation. But this surely is plainly not the case. From the judgments—A is greater than B, and B is greater than C,¹ "as they are stated," we can get only—A is greater than B, and B is greater than C; this is so in the early stages of human intelligence, and presumably also in animals; and before we can obtain from them any further knowledge—before even these judgments can become premisses—we must become cognisant (*a*) of A, B and C as common elements in a unified system, and (*b*) (to some extent at least) of the general nature of the system itself—of the characteristic relations which permeate and control it;² but such necessary knowledge cannot be obtained "from the premisses as they are stated". It is not sufficient, that is, to say that A, B and C may be taken to be "symbols standing for any magnitudes whatever" (p. 84); we must say further that A, B and C must all denote magnitudes of some one quality, which again can vary by degrees; some common character must be present in all the terms before they can enter into the inference.³ And if it be said that A, B and C need be nothing more than pure magnitudes, this merely means that they may express degrees of any such qualities indifferently; the idea of abstract magnitude, further, is difficult to form—it certainly is not "seen directly". In any case, the original premisses are transformed by receiving, either implicitly or explicitly, much wider significance. If implicitly, we must presuppose our condition, and the inference, properly expressed, becomes—

(A, B and C being elements in some magnitude system, then)
if A is greater than B, etc. ;—

or if explicitly, we get

If A, a magnitude of a quality a, is greater than
B, another magnitude of a, etc. ;—

in both cases alike we must fall back on a universal;⁴ i.e., in thus taking A, B and C as being within the magnitude system, Dr. Mercier must either bring to view an essential presupposition which the form of his original premisses conceals, or he must explicitly alter these premisses by making them more definite; in either case, he must go beyond mere "A is greater than B and B is greater than C".⁵

¹ I will admit (for the argument's sake) that these truths "as stated" can be seen "directly".

² Or (Dr. Mercier) "reveal (some) relation between A and C".

³ If, e.g., A is greater than B (in area) and B is greater than C (in weight) obviously we cannot say therefore A is greater than C.

⁴ For the distinction between the universal, and the universal judgment, see below.

⁵ If we feel we must still call the inference "direct," we can of course say that thought (owing to the immanent universal) is direct, but only as we may say that light acts on the pupil (owing to the nervous system) directly; the thought-reflex is analogous to the organic.

Any theory indeed of absolutely "direct" perception of inferred conclusions really contradicts the true nature of knowledge, and is merely a survival of those musty superstitions of "innate ideas" and the intuitive knowledge of "necessary" truths which to-day are usually confined to moral issues, "value-judgments" and doubtful theology. It makes, in short, a miracle of knowledge—analogous to the creation *de novo* of organic species—instead of regarding its growth as an ordered development through proper means; and indeed, Dr. Mercier himself, after asserting the "direct" consciousness of these conclusions, goes on to cite as their real foundation the principles of substitution and of implication; but this at once raises the fundamental questions—what ultimately justifies substitution, and what is the ultimate basis of implication? When, and why, can we truly assert or assume that A, B and C (*a*) imply each other, (*b*) in such a way that we can substitute one for the other?

It is possible, of course, to content ourselves with the mere reiteration of these underlying principles; but if we choose to go further, I think we can find their ultimate common basis only in the principle of the universal and its active function in all thought. Dr. Mercier's attitude to the universal is puzzling, and seems due to a failure to distinguish between universals, as such, and (*a*) universal judgments,¹ (*b*) the merely general. The universal itself is of course altogether different from the universal judgment; it is operative in all judgments alike (including singular and particular) and alike in all modes of inference; as to generality, the mere mention of the point is here sufficient.

If then, in saying that in A is greater than B, etc., "we see (the conclusion) directly, not indirectly through a universal" (p. 83), Dr. Mercier means "not indirectly through a universal judgment," I think his contention is correct; but if, on the contrary, he means "not indirectly through a universal," or, "through the principle or operation of the universal," he is (in my opinion) fundamentally wrong. It would, I am well aware, require at least one volume of MIND to support this view; and fortunately he makes such a task unnecessary by asserting that

(*a*) "an individual and a universal are antithetic. . . . An individual that, *qua* individual, is also universal, is a contradiction in terms," and

(*b*) "A universal, *qua* universal, can enter into only one relation, that of subsumption;" (p. 90).

(*a*) Here I think we may appeal to facts. The sole antithesis to the universal² is not the individual, but that absolute characterless homogeneity³ to which, e.g., Spencerian evolution pointed

¹ Cf., e.g., the "St. Paul's" inference (p. 90).

² To the universal, not to the universal judgment.

³ To which "abstract infinite Time and Space" approximate when we take them as devoid of all their concrete contents.

back. Consider the usual instance of "individual"—a human being—a person.¹ If we repudiate any "atomic" conception (as I presume Dr. Mercier would do) a person, obviously a complex entity, is however neither a mere group, nor a totality, nor a congeries, of its attributes, but is rather essentially a system which must be to a certain degree harmonised and self-consistent.² The point here, it must be noticed, is not mere complexity number and diversity of attributes, but the organised subordination of these into a systematic whole ; apart from this, as we often say, a person has little individuality ;³ and in general anything we call "individual" is seen on examination to have system or organisation,—in other words diversity under unity—difference under identity. The "individual" in short is what it is etymologically—the indivisible ; but indivisible at once implies diversity (you cannot divide the absolutely homogeneous) overcome by some unity which renders any real division impossible.

I think this expresses what Dr. Mercier means by "individual and universal are antithetic"—that the individual, just so far as, and the more, it is an individual, is never the general, never merely an instance of a type, a member of a class ; the more individuality a man has, the less he belongs to his party, his social grade, even his nation or his century ;⁴ and we can class any individual only by depriving it more and more of its characteristic qualities. Why then, it may be asked, if "general" and "individual" thus indicate the real antithesis, why confuse issues by introducing "universal" ? Two reasons suggest themselves :—

a. The term "individual," as commonly used, conveys a meaning approaching and suggesting not uniqueness but generality ; our "individual" is a member of society, a mere unit in the greater state ; or if not that, then the term becomes merely designative, like a proper name ; the "individual" = "the individual in question," important for some given issue—A or B or C—but apart from that, in himself alone a negligible unit ; like an obscure plaintiff raised to importance in a test case.

β . The other reason for using "universal" leads to the consideration of—

¹ The etymology of "person" seems rather suggestive in this connexion

² Otherwise we have a person who is not (*e.g.* morally and legally) a person—minors, lunatics, "multiple" personalities. But perhaps our principle is easier to grasp in the case of the bodily organism. It should perhaps be added that "system" does not necessarily mean mere mechanism and rigidity, which are merely its subordinate manifestations ; we may instance again a well-organised business or university.

³ The sometimes puzzling influence in politics and the state of "plain dull" men, and the ineffectiveness of the "brilliant," illustrate this. We may think of the late W. H. Smith and the Duke of Devonshire.

⁴ At the same time, in so far as the individual arises only under its adequate conditions, time and place must be taken into account ; and the immortals remain Greek, or English, or German.

(b) "A universal, *qua* universal, can enter into only one relation, that of subsumption."

This, to begin with, is true of the general, but only of the general, whose underlying principle is subsumption; but it is true neither (as with Dr. Mercier) of the individual, nor (as against him) of the universal; because the individual, rightly viewed and understood, is not antithetic to but is one in nature with the universal; why then, again, at all distinguish between them?

Individual and universal, argues Dr. Mercier, are antithetic, and the universal enters into the relation of subsumption; whence it would follow that the individual cannot enter into this relation. This again is true; but none the less it leaves unexpressed what is for philosophy (including logic) the most important aspect of the individual's nature. For, in so far as the individual cannot (as Dr. Mercier insists) be subsumed, just in so far is it unique; it cannot be classed just so far as it has uniqueness and stands alone—"in a class by itself" as we say. But does this character belong, except relatively, to any individual we ever experience? Dr. Mercier, I am sure, would agree that it does not—that no actual individual is altogether unique, although to be truly individual it should be unique and unclassifiable. Our actual practical "individuals" then claim, and exhibit in part, a character they do not possess in its purity. But there is more than this; not only is no individual unique, but in order to express fully such individuality as it has, it must actually recognise its own limitations—must merge its individuality in some other; it is no paradox to say that to express its individuality it must really sacrifice it.¹ Immure the most striking personality on a desert island, and his individuality is cut away at its root; he must have, *i.e.*, his proper sphere in which, by co-operation with other individualities (each of which again is in isolation helpless) he can express and develop his own. It is this fundamental aspect of the individual which is expressed by "universal"; in practice, we use "individual" (logically enough) to single out from the environment—to distinguish and isolate—thus ignoring the fact that apart from his environment the individual is helpless. "Universal" gives this neglected truth (necessary always for philosophy, but only on occasion for practice) its due prominence; it recognises that in so far as the individual cannot be classed, it is a unique system—is (though only in part) a universal; but on the other hand, in so far as it is never truly unique, but requires for its proper expression a fuller organisation environment and system beyond itself, it is not a universal, but after all only an element within some wider individual;² which again (continuing to apply

¹ "He that loseth his life . . ."

² *E.g.* family, party, church, state.

our principle) itself merges in one still wider—the only true universal being the Universe.¹

Thus the universal and the general are related to the individual in diametrically opposite ways; to generalise the individual is to degrade it—to make it wider, but shallower; while to take it up into the universal is to make it at once wider and deeper; and so far from subsumption being the only relation possible to the universal, the contrary is true; for you cannot subsume the unique; and the more an entity approaches uniqueness, the less possible does subsumption become.

If then we see conclusions "directly," this is only through the operation (implicit or explicit) of the universal; which Dr. Mercier, however, appears not to distinguish from the general; but to apply these principles in detail would necessitate writing an "Old Logic".

J. E. TURNER.

¹ "Flower in the crannied wall" is almost too well known to need quoting but the whole principle lies in it.

CAUSALITY AND IMPLICATION.

DR. BOSANQUET confines his elucidation of Dr. McTaggart to two points. I will for the sake of brevity confine mine to one of these, *viz.*, whether, and in what sense, 'Causality' is reciprocal. The general question of the validity of Dr. McTaggart's theory of causation I have examined in the *Journal of Mental Science* for last January.

Dr. Bosanquet states his question in two ways : first, whether, and in what sense, Causality is reciprocal ; and second, whether Causal Implication is necessarily reciprocal. Whether Dr. Bosanquet takes these two questions to be one and the same I do not know : to me they seem very different, and as the one that he explicitly undertakes to examine is the latter, to this I will confine myself. Dr. Bosanquet holds 'that all Implication as such is reciprocal,' and so do I ; but I am not at all sure that we both mean the same thing by implication or by reciprocal. My view is that implication is necessarily reciprocal because implication is a relation, and every relation is reciprocal, or has a reciprocal relation. This reciprocal relation is expressed by transposing the terms of the relation, and changing the verb expressing the relation from active to passive, or *vice versa*. If A loves B, then is implied the reciprocal relation that B is loved by A. If A is adhered to by B, then is implied the reciprocal relation that B adheres to A. If A implies B, then B, reciprocally, is implied by A. This is not Dr. Bosanquet's view of either implication or reciprocity. He says, 'If A coheres with, or is linked to B, B, it would seem, must be linked to or cohere with A'. It happens that both these relations can be expressed by the same voice of the verb as their reciprocals, but this is very unusual. Dr. Bosanquet takes them as if they were the rule, and the rule without any exception. If A is never found without B, he 'finds it all but impossible to conceive B, this identical B, a universal characteristic, as not possessing the corresponding feature, coupling, or point of attachment, which carries with it the presence of A'. What Dr. Bosanquet finds all but impossible seems to me sufficiently easy, unless there is some difficulty connected with 'universal characteristic,' a term of which the meaning, in this context, escapes me. If A (say treacle) is never found without B (stickiness) is it difficult to conceive B, this identical B, carrying with it the presence of jam, or honey, or glue, or syrup, and the absence of treacle? Or if the universal characteristic B is not a quality but a material thing, the difficulty is no greater. If A (a whale) is never found

without B (a remora) attached to it, still, it is very easy to conceive a remora not attached to a whale. There are many remoras attached to sharks, porpoises, and dolphins ; and some remoras are swimming free without any coupling or point of attachment to a whale. 'How,' asks Dr. Bosanquet, 'can an element [a remora for instance] united to A [a whale] without exception where A exists, cut itself loose as a whole and go about independently?' Easily enough if there are half a dozen remoras attached to A, and still A may never be without a remora where A exists. But then, Dr. Bosanquet might say, B does not cut itself loose as a whole. I agree ; but there is no need in the original supposition (A is never found without B) for B to be indivisible. It may cut itself loose piecemeal, and yet never leave A without B. In the following sentence Dr. Bosanquet assures us that, 'If our insight extends to the contrary alternative, or negative instance, and we are able to say *in addition* (my italics) "if A is not, B is not," then we are assured that B implies A, and that the relation is reciprocal'. No doubt we are, but this is not a matter of insight : it is a matter of experience—of fact. To infer deductively from 'A is never without B' to 'B is never without A' is to convert an affirmative proposition simply, and I must confess astonishment at a logician of Dr. Bosanquet's eminence falling into such a very elementary fallacy. Surely he has not been misled by the quasi-negative, 'never without,' into imagining that he was converting a negative proposition?

'Concave implies convex,' says Dr. Bosanquet. His allusion, in the previous sentence, to insight and inference leads us to suppose that the illustrative instances which follow, of which 'concave implies convex' is one, are to be regarded as inferences or deductions. It is clear to me that they are not deductions ; and if they are inductions, they are not true. 'Concave implies convex' : I deny it. In the ordinary and natural meaning of 'concave,' 'implies,' and 'convex,' this is the very reverse of the truth. In ordinary language concave means the opposite of convex, and cannot possibly imply its opposite unless by 'imply the opposite' we mean 'arouses the thought of the opposite' which is not a usual, nor, I submit, a justifiable meaning to read into 'implies'. Not concave implies, says Dr. Bosanquet, not convex. I assert, on the contrary, that not concave cannot imply not convex. It may imply convex or it may imply plane, and it must imply one or the other ; but it cannot imply not convex. Clearly, he is using the word 'implies' in some new and original meaning. What is this meaning? Fortunately he gives us a definition. 'Implication invariably means a character attaching to a definite complex of terms and relations, such that some element within it can be distinctly seen, by being what it is, to make inevitable the presence of a certain other element in a certain relation to it.' This is very widely different from Dr. McTaggart's definition of implication, and I do not see how any discussion can possibly be useful or profitable when the cardi-

nal term in it is used in totally different senses by the two chief disputants ; but taking the definition for what it is worth, as Dr. Bosanquet's definition, let us apply it to his instance. Is concave a complex of terms and relations ? To me it seems a single elementary term. Does it make inevitable the presence of convex ? To me it seems to make inevitable the absence of convex. I do not know, but as a rather wild conjecture I surmise that when Dr. Bosanquet speaks of a complex of terms and relations, he means an actually existing thing. It is a queer title for an existing thing, and it is, I admit, a daring surmise, but in no other way can I find any approach to sense in Dr. Bosanquet's statement. I surmise, however, that by a complex of terms and relations, he means, in this case, a plate of approximately uniform thickness ; and no doubt, such a plate, if concave on one side, must be convex on the other; but did Dr. Bosanquet never hear of a meniscus ? or of a plano-concave lens ? Or does he mean the concavity of a surface necessarily implies the convexity of the surface of air in contact with the concavity ? Then how if the concave surface is the interior of an exhausted receiver ? It seems to me that in every instance except that of a plate, which is a purely accidental or exceptional instance, concave does not imply convex ; concavity does not imply convexity, but is the very reverse and opposite of convexity. The same reasoning holds good, *mutatis mutandis*, of Dr. Bosanquet's other examples. Stoppage of the heart for good implies death, no doubt ; but non-stoppage of the heart does not necessarily imply the absence of death. If we take the heart out of a frog, the frog dies ; but the heart can be kept going for many hours.

The discussion of the reciprocal determination or non-determination of cause and effect hangs upon a barefaced and transparent equivocation in the word determination. In some cases, drinking alcohol determines drunkenness ; that is to say, it causes drunkenness. In the same cases, drunkenness determines having drunk alcohol ; that is to say, it is proof that alcohol has been drunk. But drinking alcohol is not proof of drunkenness, and drunkenness is not the cause of drinking alcohol. If you like to include the two words proof and cause in the meaning of the word determination, then no doubt drinking alcohol does determine drunkenness, and drunkenness does determine drinking alcohol ; but what right have Dr. McTaggart and Dr. Bosanquet thus to degrade and corrupt the English language ? Is it come to this, that philosophers have stooped to the occupation of the smasher, and debase the current verbal coin of the realm, offering us coloured German silver in exchange for sterling gold ? The instance of drinking alcohol and drunkenness is given as an instance of reciprocal determination. It is manifest that in any proper use of the words it is no such thing. If drinking determines drunkenness, then the true reciprocal is that drunkenness is determined by drinking. If drunkenness determines drinking, then the true reciprocal is that

drinking is determined by drunkenness ; and this is true in both cases whatever meanings we attach to determine, and whether we mean by it the same thing in each pair of cases or a different thing. But we must keep to the same meaning in both cases of the same pair, or we are talking utter nonsense.

Dr. McTaggart's doctrine and Dr. Bosanquet's criticism of it become intelligible only if we take 'cause,' 'imply,' 'prove,' and 'determine' sometimes all to mean the same thing ; sometimes all to mean different things ; and only if we vary the meanings capriciously from moment to moment, according to fancy.

'The whole statement,' says Dr. Bosanquet, 'to my mind, is thrown out of gear by looking at experience under the aspect of repeated conjunctions of occurrences.' The whole statement to my mind is thrown out of gear by using words in any sense that at the moment seems convenient, and changing the sense whenever the words are used again.

CHAS. A. MERCIER.

VI.—CRITICAL NOTICES.

Collected Logical Works. Vol. II. *Laws of Thought.* GEORGE BOOLE. Open Court Company. Edited by P. E. B. JOURDAIN. Pp. xvi, 448.

THIS work, which has been very rare and consequently but little read, is now being published by the Open Court Company under the editorship of Mr. Jourdain. At present only the second volume has appeared; but this contains Boole's *magnum opus*—*The Laws of Thought*—and we are promised the first volume with Mr. Jourdain's introduction shortly.

I have no hesitation in saying that this book is one of the most fascinating that I have ever read. It is a delight from beginning to end; its long period of obscurity has been a real misfortune to logic; and the Open Court Company is to be congratulated on making it accessible and putting the editorship into the hands of one whose name is a sufficient guarantee of his eminent capacity.

For a work of this kind the present volume is creditably free from misprints; but I have noticed some, and there are probably others which I have overlooked. On page 151 we read of 'the constituents in the development of y ' where y is clearly a misprint for V . In the second formula on page 286 Prob. c in the denominator should be Prob. C. In the last two equations on page 302 a symbol z occurs where, to be consistent with equation (3) on the same page, w should appear. There is a curious error on page 317. Boole is trying to find the major numerical limits of the expression $xy + x(1 - y)z$. He proves that these must be $n(x)$ and $n(y) + n(z)$ and then adds 'of these two values the last, supposing it to be less than $n(1)$, must be taken'. This must be a mistake. We must take whichever is the less of the two expressions $n(x)$ or $n(y) + n(z)$; and the fact that $n(y) + n(z) < n(1)$ does not involve that $n(y) + n(z) < n(x)$.

It is indeed easy to make up an example when this is false. Suppose that $n(1)$ is the number of male human beings, that $n(x)$ is the number of German men, $n(y)$ the number of red-haired men, and $n(z)$ the number of soldiers. Then it is tolerably obvious (a) that $n(y) + n(z) < n(1)$, and yet (b) that $n(x) < n(y) + n(z)$. Probably the true explanation of the passage is that *last* is a misprint for *least*, which saves Boole's logic at the expense of his

grammar.¹ On the top of page 323 we get the equation Min. lim. $D \geq n(1)$. It seems clear that this is a misprint for Min. lim. $n(D) \geq 0$, an equation which actually appears on the middle of the previous page.

Whilst I regard Boole's work as a great intellectual achievement, I think it is stronger mathematically than philosophically. Perhaps the most important part of it is the sketch of a general method of dealing with problems in probability. In many respects Boole's system has undoubtedly been surpassed by later logical writings such as those of Frege, Peano, Russell, and Whitehead, etc. My best plan will be to begin with a summary of the *Laws of Thought*, and then to mention some points where I disagree with it and to compare its merits and defects with those of some outstanding modern system of symbolic logic such as *Principia Mathematica*.

Logic, according to Boole, deals with the laws of our mental operations. These are determined by observation, yet our knowledge of these laws differs in kind from the knowledge of the laws of nature which we reach by observation and induction. The latter knowledge is only probable, and its probability continually increases as we become acquainted with more and more numerous favourable instances. But when we observe the operations of our own minds we become aware of a general law in the particular cases, and, once we clearly perceive it, no amount of additional instances will add to the strength of our belief. Boole does not call such knowledge *a priori*, because it does depend in a certain way on experience; but it is undoubtedly *a priori* in the sense of Kant or Meinong and in the only reasonable sense of the word. A knowledge of these laws will enable us to deal (a) with relations between things, and (b) with relations between facts or propositions. We shall thus be able to give a theoretical solution of the most general problems in ordinary logic and in probability, and we may hope in the end to obtain some light on the constitution of the human mind.

The most general problem of logic is: Given any number of relations between any number of terms $x, y, z \dots$ to deduce all that we can as to the relations between any other set of terms $u, v, w \dots$ which may or may not be wholly or partly identical with the first set. $x, y, z \dots u, v, w \dots$ may here be either simple or complex. The most general problem of probability is: Given the probabilities of any set of events subject to any set of conditions to determine those of any other set of events subject to any other set of conditions.

That logical operations can be represented by symbols is a fact which may be suspected when we recognise that all language is symbolism. That these symbols will obey laws very similar to those of algebraic symbols is a further fact which may be discovered either by considering the implications of language or by appealing

¹ I owe this conjectural emendation to Prof. Taylor.

directly to our mental operations. Thus, if single letters like x and y stand for the class of objects to which the *name* x or the *name* y is applicable; if the combination xy stands for the class of objects to which the *name* x and the *name* y are both applicable; and if the symbol $x + y$ stands for the group of objects to which the *name* x is applicable together with the group to which the *name* y is applicable, a mere consideration of the use of language will tell us that

$$xy = yx \quad \text{where } = \text{ represents identity of membership.}$$

$$x + y = y + x$$

and $z(x + y) = zx + zy$

which are perfectly comparable to the fundamental laws of algebra. It will also tell us that

$$x \cdot x \text{ (or } x^2) = x$$

a law which is peculiar to logic and is only true in algebra if x be restricted in value to 0 or 1.

Boole then proceeds to deduce these laws by direct consideration of the operations of the human mind. In his view the hearing or seeing of a general name causes the mind to turn its attention to a certain restricted group in an universe of discourse which is already before it. All the laws can be deduced from considering such operations and their combinations. He holds that in reasoning signs stand *directly* for conceptions and operations of the mind, but that, since these themselves represent things and their relations, signs *indirectly* stand for the latter. And all propositions are properly expressed by equation, in Boole's opinion; for all verbs can be reduced to the identification of two classes.

Boole makes his symbolism as like that of ordinary algebra as possible; and he does this intentionally. He says that the similarity of the formal laws, apart from questions of interpretation, is enough to justify a common symbolism. Really in short his plan is to treat logical formulæ exactly as if they were algebraical ones and to perform all intermediate processes as if this were true. At many intermediate stages this leads to logically uninterpretable equations, but at the end by subjecting the result to the condition $x^2 = x$, which differentiates logic from algebra, interpretable formulæ are obtained. Boole also takes over the numerical symbols 0 and 1, and shows that, if they are to have analogies in logic to their characteristic properties in algebra, *viz.*,

$$0 \cdot x = 0$$

$$1 \cdot x = x,$$

0 must stand for the null-class, and 1 for the universe of discourse.

He seems to hold (*a*) that the justifiability of using uninterpretable processes which lead to interpretable and true results is guaranteed *a priori* (in our sense, though not in his). That is when one has observed its success in a certain number of cases one sees that it is always justifiable, and sees this with complete certainty.

(b) He also holds that unless this procedure were justifiable there would be little use in attempting to deal mathematically with logic. (Cap. V. §§ 2-6.)

Boole's general method in logic may be summed up as follows : (a) If you are given an arbitrary function of classes x, y, \dots it may not be logically interpretable. But any statement expressed as an equation will, after certain transformations, be logically interpretable. (b) Let $V = 0$ be any equation, the left-hand side of which is of the form $\phi(x, y, z \dots)$ when $x, y, z \dots$ stands for classes and ϕ is any mathematical function. If we treat this simply as an algebraic expression in which the variables are restricted in their values to 0 or 1 we can always expand it in the form

$\phi(1, 1, 1 \dots)xyz \dots + \phi(0, 1, 1 \dots)(1 - x)yz \dots + \dots$ where the variable factors (which Boole calls constituents) consist of all the combinations that can be formed by picking out 0 or 1 or $\dots n$ of the n variables, forming their product, and multiplying it by the products of unity diminished in turn by each of the variables that has been left out in the first part of the process. The expansion will thus contain 2^n constituents, and it is obvious (1) that the product of any two of these vanishes, since any product of the form $x(1 - x)$ equals 0; (2) that the sum of all of them = 1; and (3) that they really represent a complete dichotomous division of the whole universe of discourse with respect to the properties for which the variables stand. Finally any constituent whose coefficient in this expansion is not 0 must be equated to 0, and each of these equations is a logically interpretable proposition. (This follows from the two facts (1) that the product of two different constituents = 0 and (2) that the square of any constituent is equal to the constituent.) (c) Again any explicit equation of the form $u = \phi(x, y, z \dots)$ is logically interpretable, even though $\phi(x, y, z \dots)$ itself be not so. When the right-hand side is expanded it will appear as a series of constituents whose coefficients are either 1, 0, $\frac{1}{0}$, or a , where a is any coefficient other than these and including

as a limiting case $\frac{1}{0}$. (1) The interpretation of 1 and 0 (the two coefficients which obey the law $a(1 - a) = 0$) presents no difficulty. (2) If $a(1 - a) \neq 0$ it can be proved that the constituent whose coefficient is a must be equated to 0. (This is simply an application of the fact that u , since it represents a class, is subject to the condition that $u^2 = u$) (3) The coefficient $\frac{0}{0}$ cannot be interpreted by means of mathematical deductions; it can be seen, however, that whenever $\frac{0}{0}$ appears as the coefficient of any constituent in the expansion of u the interpretation is that u contains an undetermined proportion of that constituent. To take a simple example :—

$u(1 - x) = 0$ obviously expresses the fact that All u is x .

$$\begin{aligned} \text{Solving for } u \text{ we get } u &= \frac{0}{1-x} \\ &= \frac{0}{0}x + \frac{0}{1}(1-x) \\ &= \frac{0}{0}x, \text{ when the interpretation clearly is} \end{aligned}$$

that u is identical with an indefinite part of x .

There is one further point of interest to notice here. We see that the constituents in the expansion of u whose coefficients do not obey the equation $a(1 - a)$ must be separately equated to 0. What does this mean? It means that if u , which was perhaps given implicitly in an expression of the form $\phi(x, y, z \dots u) = 0$, is to be capable of representing a class at all, certain relations must hold between $x, y, z \dots$ etc. These relations were not explicit before, but they become so when the equation is solved for u and the conditions which distinguish logic from mere algebra are imposed on the solution.

(d) Boole is now in a position to tackle his general logical problem. For this purpose two further processes are needed: (1) what he calls *Reduction*, i.e., the combination of the premises into a single proposition, and (2) *Elimination*, i.e., the removal of terms which are present in the premises but are not needed in the conclusion. It is proved that if our premises be put in the form $V_1 = 0, V_2 = 0 \dots V_n = 0$, then the equation $V_1 + c_2V_2 + \dots + c_nV_n = 0$ (when $c_2 \dots c_n$ are arbitrary multipliers) gives all the information provided by the separate premises and no more. Again if the coefficients of the constituents in the expansions of V_1, V_2, V_n be all positive, the coefficients $c_1, c_2 \dots c_n$ can all be reduced to unity, and $V_1 + V_2 + \dots + V_n = 0$ will give all and no more than all the required information. Lastly, if these coefficients be not all positive it is only necessary to square each of the equations and add. So that $V_1^2 + V_2^2 + \dots + V_n^2 = 0$ will always have just the combined force of the premises $V_1 = 0, V_2 = 0 \dots V_n = 0$. (These results are once more a consequence of the fundamental facts that if t_m and t_n be any two constituents $t_m t_n = 0$ and $t_m^2 = t_m$ and $t_n^2 = t_n$.)

Elimination, as Boole carefully points out, is considerably different in logic and in algebra. In algebra the number of terms that can be eliminated depends on the number of independent equations between them that are given. But in logic elimination is conducted by means of the fundamental equation of duality $x^2 = x$, and so any number of terms can be eliminated even from a single equation. (The only limitation is that, if you try to eliminate so many terms that your original data supply no information as to the relations between those that are left, you will be confronted with the platitude $0 = 0$.) The result of eliminating x from

$\phi(x) = 0$ is $\phi(1) \cdot \phi(0) = 0$. That of eliminating x from $\phi(x, y) = 0$ is $\phi(1, y) \cdot \phi(0, y) = 0$. That of eliminating x and y from $\phi(x, y, z) = 0$ is $\phi(1, 1, z) \cdot \phi(1, 0, z) \cdot \phi(0, 1, z) \cdot \phi(0, 0, z) = 0$.

The general rule can easily be seen from these examples. The proof depends on expansion in constituents and application of the Law of Duality $x(1 - x) = 0$.

(e) The solution of Boole's general logical problem is now all over except the shouting (which in this case consists of certain methods for abbreviating the process described above). The problem is : Given premises involving classes $x, y \dots$ to find all that can be discovered from them about any class u which is any function of the classes $z, w \dots$ (It is not necessary that $z, w \dots$ etc., should explicitly be included among the $x, y \dots$ of the premises, for they can always be introduced on expansion in constituents, e.g., $x = xw + x(1 - w)$.) The solution of the problem is : (1) Reduce the premises to a single equation $\phi(x, y \dots) = 0$. (2) If $\psi(z, w \dots)$ be the required function put $u = \psi(z, w \dots) = 0$. (3) Reduce these two equations to a single one of the form $\chi(x, y \dots; z, w \dots; u) = 0$. (4) Eliminate $z, w \dots$ from this. (5) Solve the resulting equation for u . You will thus obtain u as an explicit function of constituents involving $x, y \dots$.

This will be an interpretable proposition, and any necessary conditions among the variables $x, y \dots$ will become explicit. In Chapter IX. Boole gives various methods by which these processes may be shortened. They consist essentially in recognizing the simplifications which the Law of Duality imposes on algebraic expressions. Thus our old friends

$$\begin{aligned} pVp &\equiv p \text{ and} \\ (pVq)(pVr) &\equiv pVqr \text{ and} \\ pVpq &\equiv p \end{aligned}$$

appear here under thin disguises. What this chapter really tells us is that it is often useful even for practical purposes *not* to wait till the end of a process before imposing the conditions that differentiate logic from algebra. As we shall see later, this is rather an important admission.

The next important point to notice is Boole's distinction between primary and secondary propositions and his method of dealing with the latter. A primary proposition for Boole is one which makes an assertion about things, e.g., *Cæsar crossed the Rubicon*, *All men are mortal*, etc. A secondary proposition states a relation between facts, e.g., *If it rains I shall get wet*, *Either he will arrive by 2.30 or I shall go out*. Not all propositions in the hypothetical or disjunctive form are secondaries. Boole calls : *Animals are either rational or irrational* primary. And not all secondaries, according to him, are hypothetical or disjunctive. *It is true that Smith is a knave* would be a secondary proposition. Boole treats all secondary propositions as referring to time. Let $X, Y \dots$ be primary propositions. Let x be the class of moments at which x

is true ; similarly for y , etc. Let 1 stand for the whole time under consideration. Then (a) X is true can be expressed by $x = 1$, and x is false by $x = 0$. If Y then X can be expressed by $y = vx$ when v is an indeterminate class of moments which may have any value from 0 to 1. Either Y is true or X is true can be expressed by $y + (1 - y)x = 1$. (b) Equations containing x , y , obey all the laws of primary propositions, and in our work we can forget their reference to time and act as if we were dealing solely with primary propositions.

Boole's book teems with examples fully worked out, which are of great use to the student. He devotes two chapters (XIII. and XIV.) to a full treatment of certain arguments used by Clarke and Spinoza. There is also an interesting chapter (XV.) on Aristotelian Logic. Boole easily deduces the rules for immediate inferences and for the syllogism on his principles. The syllogism is essentially a method of elimination. Boole argues that probably all elimination could be reduced to syllogism, but that the general problem of logic is not merely one of elimination but is the one which he has solved ; and that the solution of this general problem cannot be performed by the doctrine of the syllogism alone. Moreover, he says, whilst such principles as the *Dictum de Omni et Nullo* are self-evident, they are not fundamental. They, together with much else which cannot be deduced from them, can be deduced from more primitive principles. He is inclined to make the Law of Contradiction the fundamental principle of logic. This is apparently because we have constantly used the Law of Duality, $x^2 = x$, and because this can be written in the form $x \cdot x = x \cdot 1$, whence $x \cdot x - x \cdot 1 = 0$ or $x(1 - x) = 0$. And this, on interpretation, becomes : Nothing is both x and not x .

It will be convenient to criticise Boole's logical doctrines before passing to his theory of probability. The latter is based on the former, but involves additional elements which will need to be explained and criticised later.

(1) Is logic really the science of the laws of our mental operations? Boole continually speaks as if it were. Yet he certainly does not confuse it with empirical psychology, since he holds that the truth of its laws is seen in their instances, not merely rendered probable by induction. And, as far as I can see, the only positive argument that he produces for thinking the laws of logic to be laws of our mental operations and not laws of their objects is that such an axiom as

$$xy = yx$$

involves a difference of order which is present among our acts of thought, but is not present among their objects. To me it seems clear that this argument does not show that the laws of logic are laws of our mental operations, and that the truth is that they are the laws of certain objects, *viz.*, propositions, their parts, and their relations.

It is true that these objects are essentially objects of *thoughts* (as distinct from other mental states such as sensation and perception), and further that the relation which subsists between the objects of certain acts of thought (e.g., in the case of inference) determine whether these acts can be described as *valid* or *justifiable*. But this seems to be the sole special connexion between logic and thought, and it evidently does not make the laws of logic laws of thinking. As to the equation $xy = yx$ the truth seems to be as follows: As a matter of physical fact two symbols x and y can be written in two different orders; as a matter of psychical fact two classes can be thought of successively in two different orders; as a matter of logical fact the symbols xy and yx stand for one and the same class. The law $xy = yx$ is therefore a statement that there is no difference among *logical* objects to correspond to the difference of *spatial* order among symbols, or to the difference of *temporal* order among acts of thinking. So far then from being purely a law of thought, as Boole suggests, the identity asserted by it can only be understood if we go outside the different and successive acts of thinking and consider their single and timeless logical object. (Similar considerations would show that it cannot be an assertion purely about symbols.)

(2) Should propositions be expressed by equations? In the main undoubtedly Boole's motive for expressing propositions as equations was to obtain as much analogy as possible with ordinary algebra. The same may be said of his treatment of secondary propositions. A logician who is breaking new ground in formal logic will always be torn between two ideals: (*a*) that of recognising every distinction among propositions and of analysing the different kinds as fully and accurately as possible, and (*b*) that of establishing a symbolism which shall be as simple and fruitful as possible.

Frege and Boole illustrate the striving after the first and the second of these ideals respectively, and it is the merit of Russell's and Whitehead's system to hold the balance very evenly between them. In general Boole does not pretend that an equational representation is an adequate analysis of all kinds of propositions, yet he does seem to offer one argument. In trying to show that all verbs may be replaced by = and a class-symbol he argues as follows: You cannot understand the proposition *Cæsar conquered the Gauls* unless you understand what is meant by *One who conquered the Gauls*. Hence the latter is an essential constituent of the former, and the proposition really means (and is not merely logically equivalent to) *Cæsar is identical with one who conquered the Gauls*. The error of this analysis seems to me to be that it overlooks the important fact that a *finite* verb has two logical functions. As a *verb* it represents a relation, the same relation as its infinitive stands for. As a *finite* verb it also makes an assertion, the sign of which is the verbal inflexion. Now the fact of assertion is indeed common to all propositions whatever, and could be re-

presented by a common word or symbol the same for all propositions. But the relations represented by different verbs are different from each other and different from that represented by the verb *to be* (as used in *Cæsar is mortal*). If you force the verb into the predicate as in *One who conquered the Gauls* you have made no analysis whatever. You have (a) to recognise that this is at least a very different kind of predicate from *mortal* or even from *one who is mortal*; and (b) that it involves a relation between terms which is not that of identity. The verb has merely gone into the grammatical predicate, and any attempt to get rid of it there in favour of the relation of identity will only start you on an infinite regress. To put it generally, the notion of *x*'s being mortal seems to be logically prior to the notion of *an x such that x is mortal* and it is therefore perverse to offer *Smith = one who is mortal* as an analysis of *Smith is mortal*; and further, even if this analysis were valid, it is mere lack of thought to treat *Cæsar conquered the Gauls* as if it were probably similar to *Cæsar is mortal*.

(3) There is nothing then to be said for equations as an *analysis* of propositions in general. Can we say that the advantages of making formal logic as analogous as possible to algebra outweigh the disadvantages? The only advantage that I can see is that elementary algebra and its symbolism is familiar to all educated people. Against this we may set the following disadvantages: (a) As we have seen an equational system necessarily involves a divorce between formal development and philosophical analysis. (b) Experience shows only too clearly how liable the practice of using the same symbols to represent different kinds of objects is to lead to hopeless confusion, *viz.*, to the failure to recognise that the objects denoted by the same symbols are different. The sign 2 unfortunately represents the integer 2 , the rational fraction $\frac{2}{1}$, and the real number 2 (*i.e.*, a class of rational fractions). These are utterly different things; but, owing to the fact that they are all represented by the same sign, it is extremely difficult to get most people to see that they differ. (c) If formal logic be used (as in *Principia Mathematica*) for proving the fundamental laws of arithmetic, and, more generally, if we want to determine the relation of logic to mathematics, our enquiry will be confused and prejudiced at the outset by using in logic the symbols of arithmetic. (d) Since the mathematics of logic is simpler than ordinary algebra (owing to the existence of the relation $x^2 = x$ in logic) it is very perverse to insist on pretending all through one's work that this simplification is absent and only to impose it at the end. Boole himself practically admits this when he introduces his chapter on *Methods of Abbreviation*. (e) If we work with implications instead of equivalences we can always get back to equivalences if we want them by using the two equations

$$\begin{aligned} p)q \cdot \equiv & : p \cdot \equiv . pq \\ \text{and } p)q : \equiv & : q \cdot \equiv . pVq. \end{aligned}$$

(4) A defect in Boole's logical symbolism is his treatment of particular propositions. We very greatly miss the symbols $(\exists x)$ and (x) of Russell and Whitehead. Primarily letters in Boole's system represent classes; thus we may compare his x 's and y 's to Russell's and Whitehead's a 's and β 's. But, owing to his having no symbol for class-membership, no symbols for individuals, no incomplete symbol like $(\exists x) \dots$, and consequently no expressions of the form $(\exists x) . xea$, he is faced with the following problem: He must express *particular* propositions solely by relations of *equality* among *classes*. To do this satisfactorily is almost impossible. Schröder,¹ whose system resembles Boole's in many respects, used inequalities. But Boole does not do this in his logic, presumably from his desire to keep as close to ordinary algebraic equations as possible. He thus expresses All y is x by the equation $y = vx$, which means: The class y is identical with the common part of the class x and some indeterminate class v . What Boole really wants to say is that All y is x is equivalent to the statement: There is a class v such that $y = vx$. But he has no means of symbolising this kind of statement. In Russell's and Whitehead's notation it would be expressed in the form $(\exists v) . y = v \sim x$; and this is formally equivalent to $y(x)$. Having no symbol such as $(\exists v)$ Boole is compelled to introduce his indefinite class-symbol v as a real variable instead of an apparent variable. There is nothing in the nature of his symbolism to show that v , rather than x or y , stands for: *There is a class v such that . . .*, and that the statement $y = vx$ is not about the class v in the same sense in which it is about the classes x and y .

This defect is not very important in dealing with A propositions, because, as Boole points out, v can be eliminated and All y is x can be expressed by the equation $y(1 - x) = 0$. But this excuse cannot be made for his symbolism for I and 0 propositions. He symbolises Some y is x by the equation $vy = vx$. Allowing that v may be interpreted as *There is a class v such that . . .*, this means: There is a class such that its common part with x is identical with its common part with y . But this will always be true; for, if v be the null-class, we have $y.0 = x.0$ whatever x and y may be. We ought therefore at least to add the statement that $v \neq 0$. Hence a particular proposition cannot be expressed by an *equality* among classes alone. Again, we might enquire why v should appear on both sides. Would not the equation $vy = wx$, i.e., There is a class v whose common part with y is identical with the common part of x and some class w , be more general? Take: Some men are black. If this be true is it certain that there is any one class except the null-class and the class of black men such that its common parts with x and with y are identical? Neglecting

¹ Schröder and Couturat have also incomplete symbols Π and Σ to correspond to $(x) \dots$ and $(\exists x) \dots$.

the first case, for the reasons given above, the second would reduce the equation to the tautology $xy.y = xy.x$

i.e.,

$$xy^2 = x^2y$$

or

$$xy = xy \text{ by the Law of Duality.}$$

To illustrate the same point and to give an example of Boole's methods we may solve the equation $vy = vx$ for y . We have

$$y = \frac{v}{v}x.$$

$$\text{Whence } y = vx + (1 - v)x \cdot \frac{0}{0} + (1 - v)(1 - x) \cdot \frac{0}{0}.$$

Here, it will be observed, a new indeterminate class symbol has been introduced by the coefficient $\frac{0}{0}$. And it cannot in general be identical with v itself, or the equation would reduce to $y = vx$ which represents All y is x .

As Boole points out v cannot be eliminated from $vy = vx$, for the attempt leads to the platitude $0 = 0$. As he also points out either v or w but not both can be eliminated from $vy = ux$, the result being, e.g., $vy(1 - x) = 0$. This form of the equation is also open to the criticism mentioned above that, unless the inequality $v \neq 0$ be added, it does not properly represent a particular proposition. In fact a comparison of this form of the equation with the equation $y(1 - x) = 0$, which represents an A proposition, shows very clearly that Boole is trying to represent an I proposition by an A proposition; for $vy(1 - x) = 0$ means literally All vy is x , and, allowing for Boole's failure to symbolise *There is a v such that . . . ,* means: There is a v such that all vy is x . This must be admitted to be a very clumsy and unnatural way of symbolising *Some y is x*.

There is one other point to notice before leaving this subject. When, in the solution of an equation, Boole gets several constituents each provided with the coefficient $\frac{0}{0}$, he tells us that we may

add the constituents as they stand and prefix $\frac{0}{0}$ to the result. The

reason given is that $\frac{0}{0}$ may stand for *any* class. This is surely a bad reason. If it may stand for *any* class we are not justified in assuming that it stands for the same class in each case; and unless this be assumed it is not obvious why we may take it outside the sum as a single logical factor. Boole's procedure is, however, really justifiable. Suppose we have such an equation as

$$y = \frac{0}{0}x + \frac{0}{0}(1 - x)z$$

or, as it might be written, $y = vx + w(1 - x)z$. The question is whether we are justified in writing this in the form

$$y = w(x + \overline{1 - x}z) \text{ or } y = \overline{0}(x + \overline{1 - x}z).$$

Boole could have proved the justifiability of this procedure in the following way: If we put $y = vx + w(1 - x)z$ and $t = x + (1 - x)z$ we can form a single equation. If we eliminate from this v , w , x , and z we shall find ourselves left with the equation $y(1 - t) = 0$, which, on solution, gives $y = \overline{0}t$, i.e., $y = \overline{0}(x + \overline{1 - x}z)$. The very fact, however, that there is an apparent difficulty here shows clearly that symbols like u , v , $\overline{0}$ are not ordinary class-symbols like x , y , z . . . , but are a very awkward and inadequate way of symbolising what Russell and Whitehead denote by the incomplete symbol $(\exists u)$ Thus the proposition $y = vx + w(1 - x)z$ is really only adequately symbolised by the expression

$$(\exists v, w). y = vx + w(1 - x)z.$$

(5) The last point that I shall criticise before leaving the purely logical part of the work is the distinction between primary and secondary propositions and the introduction of time. In the latter point Boole is once more followed by Schröder, and it seems to me that, apart from all special arguments, a comparison with the respective fates of Fluxions and the Differential Calculus is ominous for this procedure. There is undoubtedly a genuine distinction between primary and secondary propositions, and Boole's distinction partly coincides with it. A proposition which asserts a quality of a proposition or propositional function, or a proposition which asserts a relation between two propositions or propositional functions, may fairly be called secondary. Thus p is true, p is necessary, $p)q$, $\phi x)_x\psi x$, and $(x). \phi x . (x). \psi x$ are secondary propositions. Now Boole so far agrees with this as to call secondary (a) propositions which ascribe the quality of truth or falsehood to propositions, and (b) those which assert a relation of disjunction or implication between two propositions (e.g., what Keynes calls 'True Hypotheticals'). Thus he would count as secondary: *If it rains I shall get wet* and *If everybody be unvaccinated somebody will have small-pox*. But (c) he does not count as secondary propositions of the form $\phi x)_x\psi x$, i.e., what Keynes calls 'Conditionals,' nor the corresponding disjunctives. Thus he would count as primary the proposition *If anyone be unvaccinated he will have small-pox*. There seems to be no good ground for this distinction, and Boole's error doubtless arises from the fact that he did not clearly recognise the distinction between propositions and propositional functions, and between real and apparent variables.

If he had carried his analysis further and declined to regard equations expressing identity between classes as ultimate, he would

have seen that primary propositions are really by no means common, and that the greater number of his so-called primary propositions are really assertions about the formal equivalence of functions.

We may now turn to Boole's doctrine of the connexion of secondary propositions with time. Boole seems to regard propositions asserting relations between events as the type of secondary propositions. Now these do contain an essential relation to time. But when he tries to make propositions like p is true refer to time his doctrine loses its plausibility. It loses it still further when we remember that a vast number of hypothetical propositions are not about events at all but about essentially timeless objects. Take the proposition if $3 > 2$ and $2 > 1$ then $3 > 1$. It is surely preposterous to offer as the meaning of this : The class of moments at which it is true that $3 > 2$ and that $2 > 1$ is identical with some part of the class of moments at which $3 > 1$. The absurdity is due to the fact that objects like 1, 2, and 3 are timeless, and the relations between them are timeless too.

Boole explicitly identifies eternal truths with propositions which are true at all times. This appears to me to contain a double error. (a) All propositions, if true at all, are true independently of time. When we say that a proposition about x is sometimes true we mean that a function involving x and t gives true propositions for certain values of t . This is disguised by the facts (1) that all assertions about events really involve a reference to the time at which they happen, and (2) that this reference is often not made explicit in speech and writing. Thus *Queen Anne is dead* seems to stand for a proposition and to be true at some times and false at others. But the fact is that, since the death of Queen Anne is an event, this form of words is incomplete, for it contains no explicit reference to time. The same form of words as used by me and as used by William III. do not stand for the same proposition, and therefore the fact that my statement would be true and William's verbally identical statement false does not prove that any proposition has been false and has become true. (b) A proposition which is 'always true' is an assertion that a function involving time gives true propositions for all values of t . Thus the proposition *If amber be rubbed with silk it becomes electrically charged* means *If at any time amber be rubbed with silk it then becomes charged at that time.* Such propositions are always about events. An eternally true proposition is one about the timeless qualities or relations of timeless objects. The whole of pure mathematics and logic provides an example of this.

Boole's own treatment of the relations of propositions to time seems to me very unsatisfactory and confused. He writes for X is true $x = 1$, i.e., The times at which X is true are all times. But he also holds that a proposition may be sometimes true and sometimes false. How can this be compatible with the above

notation for X is true? I suppose the solution is that for Boole X is true has two senses. (a) It is an incomplete symbol which only stands for a proposition when a temporal determination is added. (b) It is this with the determination at all times added. He nowhere gives an expression for X is sometimes true. I suppose it would have to be $x = v$ and $v \neq 0$.

Let us now pass to Boole's general method in probability. As before we will first state and then criticise. According to him probability may be approached from two different points of view; each will lead to the same numerical results, and each in the end needs to be supplemented by the other. The first method is to define probability fractions as the ratio of the number of cases that give true values to a given propositional function (Boole does not of course use this expression) to the total number of cases, assuming them all to be equally likely. With this definition we can prove the usual fundamental propositions about the probabilities of conjunctive and disjunctive propositions, and we shall find that the probability of any event compounded in any way of the simple events x, y, \dots is the same *algebraical* function of their separate probabilities p, q, \dots as the compound event is a *logical* function of the events x, y, \dots . The other method of attack is to start by assuming that expectation is a state of mind which, although it cannot be accurately measured, is at least subject to certain rules of increase and decrease. If we now assume that the measure of the expectation of a complex event is the same *algebraical* function of the probabilities of the separate events as the expression for the complex event is a *logical* function of the separate events, we find (a) that what common-sense judges to be greater or less degrees of expectation will have greater or less measures respectively, (b) that certainty is expressed by 1, and (c) that the ordinary laws of probability follow.

We now come to Boole's solution of the general problem. By 'the event x ' he means 'that event of which the proposition which asserts the occurrence is expressed by the equation $x = 1$ '. And similarly for compound events. Events are 'conditioned' when they are not free to occur in every possible combination; otherwise they are unconditioned. If now $\phi(x, y, z) = 1$ represents a compound event; x, y, z represent simple *unconditioned* events; and the probabilities of x, y, z , etc., are p, q, r, \dots respectively, then Prob. $\phi(x, y, z) = \phi(p, q, r, \dots)$ when the +'s, \times 's, etc., in the first are to be read in their logical sense, and in the second in their arithmetical sense. Next Boole determines the *unconditioned* probabilities of a number of simple events given their probabilities under a condition $V = 1$. Now let x, y, z be any simple events; let S, T, \dots be any compound events which are logical functions of these, and let us try to find the probability of any other compound event W . We can form a logical equation expressing W in terms of constituents formed from S, T, \dots , regarding these as single logical terms. It will take the form

$$w = A + 0B + \frac{0}{0}C + \frac{1}{0}D$$

when A, B, C, D are sums of constituents involving s, t, . . . etc. (Here w, s, t . . . are letters written instead of the complex functions W, S, T, etc. What we do is to write s = S, t = T . . . , w = W; reduce these to a single logical equation, and eliminate x, y, . . .) The solution of the above logical equation is

$$w = A + qC$$

and

$$D = 0.$$

The latter is a condition independent of w and may also be written in the form A + B + C = 1, or, for shortness, V = 1.

We now wish to pass from logic to probability. We were given the probabilities of s, t, . . . ; but the condition V = 1 has emerged as involved in our data. Hence the given probabilities were probabilities *subject to the condition* V = 1 and not the probabilities of s, t, . . . as *unconditioned* events. We cannot therefore pass at once from logic to algebra, but must first find the *unconditioned* probabilities p^1, q^1, \dots of these events by the method which Boole has already given us. If we substituted these values straight away on the right-hand side of our equation, we should get the probability of w as an *unconditioned* event; but w is not unconditioned for it is subject to the condition V = 1. Hence we really require to find Prob. w under the condition V = 1. This Boole shows to be equal

to $\frac{\text{Prob. } Vw}{\text{Prob. } V}$. Hence:

$$\text{Prob. } w = \frac{\text{Prob. } V(A + qC)}{\text{Prob. } V} = \frac{\text{Prob. } (A + qC)^1}{\text{Prob. } V}.$$

The right-hand side can now be determined by substituting the values p^1, q^1, \dots for s, t, . . . respectively everywhere in it, and reading all *logical* +'s, ×'s, etc., as *algebraical* ones. (I should say that Boole's exposition here is very condensed, and, to me, hard to follow. I think I have understood it, but I have added several steps that seem to me (a) justifiable, and (b) necessary for clearness.

Boole solves the still more general problem when the probabilities of S, T, etc., are given not *simpliciter* but under an explicit condition. No additional difficulty in theory is involved here since the explicit condition can be dealt with just like the originally implicit one which became explicit in the solution of the simpler problem.

One further question remains if this general method is to lead to determinate results in all cases. In passing from conditioned to the corresponding unconditioned probabilities we may have to solve algebraic equations of a degree higher than the first. We may then be in doubt as to which root to take. In a very

¹ For A, B, C, and D can contain no constituents in common and products of different constituents will vanish.

brilliant chapter on *Statistical Conditions*, Boole shows us how to determine this question. Incidentally in this chapter he gives all that is required for solving problems on Numerically Definite Syllogisms, as De Morgan called them. Examples are then supplied of the general method in two excellent chapters (XX. and XXI.) dealing with Problems on Causes and the Probability of Judgments.

I regard this work of Boole's on probability as being of the utmost brilliance and importance. I am not aware that the general problem which he solves has been solved before or since. So far as I can judge Boole's solution is essentially sound, and perhaps the very neat relation which appears in it between logic and algebra is a good excuse for approximating the two symbolisms, at any rate when dealing with problems on probability. On certain points, however, I find a good deal to criticise.

(1) Boole constantly talks of the probability of a proposition. I am sure that this is meaningless or elliptical. Probability is always probability relative to some datum or other. Perhaps the probability of a proposition might be interpreted as its probability relative to the laws of formal logic and to no additional propositions; this seems to be what Boole means by unconditioned probability.

(2) Boole confuses two apparently similar but really very different notions, *viz.*, *The probability of p given q* (which, following Mr. W. E. Johnson's convenient notation, we will write p/q) with *The probability of if q then p*. Interpreting the probability of any proposition as its probability relative to the laws of formal logic, and denoting the latter by f , this would be written $[q]p]/f$. Now the two are quite different. One is the probability that p is true given that q is true; the other is the probability that q implies p given the laws of formal logic. The cause of the confusion is the following: If we forget that the probability of a proposition in itself is meaningless we are liable to think that *The probability of (p if q)* is the same as *(The probability of p) if q*. And this is what Boole does. It leads him to one very extraordinary conclusion which he himself recognises to be paradoxical and which I regard as in itself a sufficient refutation of his theory. He shows that, on his theory, two formally equivalent propositions will have two different probabilities. The example that he takes is *If x then y* and *Either y is true or both x and y are false*. If the probability of the second be p he proves that that of the first will be $\frac{cp}{1 - p + cp}$ when c is an undetermined constant. Now this result follows through his taking Prob. (*if x then y*) as $\frac{\text{Prob. } xy}{\text{Prob. } x}$, i.e., taking $[x]y]/f$ as the same as $\frac{xy/f}{x/f}$. But the fact is that they are not equal. The latter = $y/x/f$, i.e., the probability of y given x and

the laws of logic. The former may be determined by the following considerations :—

$$x)y \cdot \equiv . \text{not}(x \text{ and not } -y).$$

∴, assuming that formally equivalent propositions have the same probabilities relative to the same data,

$$\begin{aligned} [x)y]/f &= \text{not}(x \text{ and not } -y)/f \\ &= 1 - x\bar{y}/f \text{ (writing } \bar{y} \text{ for not } -y) \\ &= 1 - x/f \cdot \bar{y}/xf \\ &= 1 - x/f(1 - y/xf) \\ &= 1 - x/f + x/f \cdot y/xf \\ &= 1 - x/f + xy/f. \end{aligned}$$

If we use this value and apply Boole's methods we shall find that $[x)y]/h$, when h is the proposition that $[y + (1 - x)(1 - y) = 1]/f = p$, is equal to p .

(3) I now pass to a point of interpretation where I find Boole very difficult to follow. When we solve our general logical equation we get

$$w = A + qC$$

where q is an indeterminate class. When we pass from logic to algebra Boole writes an indeterminate probability c for q . So far all is clear. Then he proceeds to interpret c . I quote his argument (p. 283): 'The logical equation, interpreted in the reverse order, implies that if either A take place or C in connexion with q , w will take place, and not otherwise'. (This is obviously true.) 'Hence q represents the condition under which, if C take place, w will take place. But the probability of q is c . Hence, therefore, c = probability that if C take place w will take place.'

Now I cannot accept the latter part of this argument. We have proved (a) that $qC)w$, i.e., that $q \cdot C)w$. And (b) we are told that $q/f = c$. But the probability of an implied proposition is *not* the same as that of one which implies it on the same data. Suppose, e.g., that $x)y$; let us call this datum h . Let $x/h = p$, and let us try to find y/h . We have

$$\begin{aligned} y &= yx + y\bar{x}. \\ \text{Hence } y/h &= yx/h + y\bar{x}/h \\ &= x/h \cdot y/xh + (1 - x/h)y/\bar{x}h \\ &= x/h + (1 - x/h)y/\bar{x}h \\ &= p + (1 - p)q \text{ when } q = y/\bar{x}h. \end{aligned}$$

Hence it does not follow from the facts that $q \cdot C)w$ and that $q/f = c$ that $[C)w]/f = c$. If, instead of $q \cdot C)w$, we had $q \cdot \equiv . C)w$ the required result would be obtained. But we do not have this. If we did we should have to have $C)w \cdot . q$. Now this would imply $w)q$, which is certainly not in general true.

And if we look further into Boole's statements on page 283 we cannot feel sure that he really means to assert that $c = [C)w]/f$.

For he proceeds to add that $c = \frac{Cw/f}{C/f}$. These two statements, as

we have seen, are not equivalent; though Boole thought they were. Hence we cannot be sure which of the two he means. I am pretty clear, however, that he means the second. In the first place in the simple example (1) worked out by Boole on page 293, we can see that $c = \frac{Cw/f}{C/f}$. Secondly, I offer with some diffidence the following

general proof that $c = \frac{Cw/f}{C/f}$. In the equation $w = A + qC$

multiply both sides by C, remembering that CA = 0, and $C^2 = C$. We get $Cw = qC$. Hence $Cw/f = qC/f$. Now, if q and C be independent, $qC/f = q/f \cdot C/f$. But q is a purely arbitrary proposition; hence its probability cannot be affected by the truth or falsity of C; hence we may treat q and C as independent. We thus get the equation

$$\begin{aligned} Cw/f &= q/f \cdot C/f \\ \text{i.e. } c &= q/f = \frac{Cw/f}{C/f}. \end{aligned}$$

(4) I find Boole's notation for simple events and conjunctions of simple events far from satisfactory. If x represents the event of raining the proposition *It rains* will be represented by $x = 1$. Similarly if y be the event of thundering, the proposition *It thunders* is represented by $y = 1$; the event xy is the double event of thunder and rain; and the proposition *It thunders and rains* is expressed by $xy = 1$. But if Boole is keeping to his notation for secondary propositions these equations surely ought to stand for the propositions: *It is always raining*, *It is always thundering*, and *It is always raining and thundering* respectively. The fact is that he does not provide a satisfactory notation for the two very different propositions: *It is true that it rains* and *It is always raining*. His failure to provide any notation at all for singular propositions (which, I am afraid, comes from a failure to distinguish the two relations ϵ and $)$) is also very inconvenient in dealing with many problems of probability. Nevertheless, I believe that Boole's mathematical treatment of probability is a great and original achievement, and that it would be easy and thoroughly worth while (when we have finished saving civilisation by the mutual slaughter of almost everyone who makes the continuance of civilisation possible) to remove its errors of detail.¹

I conclude with a few words on Boole's views as to the light that mathematical logic throws on the constitution of the human

¹ I have now (Nov. 1916) succeeded in doing this and in giving a satisfactory account of $\frac{0}{0}$ and C. The work contains too many symbols for its publication in a periodical, so the reader must take my statement on trust for the present.

mind. His most characteristic doctrine is that, whilst the fact that the laws of thought and the laws of matter are mathematical in form might induce us to suppose that mind as well as nature is governed by necessity, the further fact of error shows that this conclusion is unwarranted and that either the mind can break the laws of thought or at least that these laws are only part of a much larger system of laws and may be suspended in the same kind of way in which you may say that the law of gravitation is suspended by the Principle of Archimedes in the case of a Zeppelin. To me there appears to be little of importance in these reflexions, because, as I have tried to argue, the laws of logic are not even a part of the laws of thinking but are the laws of certain objects which can only be grasped by thought.

C. D. BROAD.

Religion and Science: a Philosophical Essay. By JOHN THEODORE MERZ. Edinburgh and London: William Blackwood and Sons, 1915. Pp. xi, 192.

No one has laid the English student of modern thought under heavier or more varied obligations than Dr. Merz, whose sympathetic knowledge is as much in evidence in the present Essay as ever, with the advantage of being set forth in a style of exposition if possible still more lucid and equable than before. He has addressed himself to "the increasing class of thoughtful persons, especially among the younger generation . . . who feel themselves sore perplexed by the contradictions which apparently exist between the dicta of science and the tenets of religious creeds, who are not prepared to sacrifice the truth of either, but who find it extremely difficult to reconcile them" (p. 4).

The brief work is described in the sub-title as "philosophical," but more than once in the text the epithet "psychological" is used rather markedly, as on p. 166, where we read, "the psychological theory developed in the foregoing pages". Not only so, but after an introductory discussion of the ordinary popular view of the outer and the inner world—the view, that is to say, which contrasts these two worlds and puts them in opposition to each other, like the image in a mirror facing its original—Dr. Merz argues that they may better be regarded as "lying, as it were, on the same plane, making up together the total field of our consciousness". Immediately afterwards this is designated the "exclusively introspective point of view"; and the opinion is expressed that the advance of philosophic thought has been retarded by the difficulty of confining oneself strictly to introspective data, though British philosophy more than any other has tended to revert to the true path. And the object of the Essay is stated to be that of applying "this purely introspective view" to a special problem—the problem of Religion.

It is obvious that grave difficulties are involved in this general

method. To mention only two—we shall have constantly to be on our guard against a tendency to desert the standpoint of psychology and take up questions of validity and value, on which introspection can have nothing important to say. And further, it is quite certain that introspection as a method can do no sort of justice to the relation, which to the present writer seems vital, between Religion and History. Whether or no it is owing to these intrinsic difficulties of procedure, I freely confess to having found the course of Dr. Merz' argument as a whole, somewhat hard to grasp and I shall strive to lessen the injustice I seem bound to do it by stating it as far as possible in his own words.

The Essay is in three parts. Part I., more general in character, deals with "some of the more important features in the formation of our full-grown view of the world". These pages are very fresh and for the most part convincing, if taken, as the writer means them to be taken, as psychology. All our knowledge of external things is a development and elaboration of what were and still are purely subjective experiences; it is for each of us individually comprised in the moving stream of thought or (to use a figure that Dr. Merz prefers) the firmament of the soul. Appeal for the truth of this is made to an imaginative reconstruction of infantile experience. One fundamental notion with which the developing mind has to become familiar is that of Reality. This it actually reaches through the influence of personality or personalities—the mother, for example. "The instant at which the figure of a person flashes, as it were, on the background of the mind's consciousness, is the moment of birth of the distinction between object and subject" (p. 41). And the first acquaintance with anything real invariably grasps a totality, a "together". Thus the primordial view is synoptic, and this whole, lost in subsequent analysis, can never be recaptured with certainty by synthesis. The objective world, then, is not a continuum, as the stream of thought is, but a more or less disconnected assemblage of special experiences which have certain attributes in common. As Dr. Merz puts it in an important passage: "In later life, and especially in scientific research, we try to restore that continuity which the things of the objective world, in consequence of their detachment from the continuous background of consciousness, have lost" (pp. 53-4). The continuum had to be broken up for intellectual progress. At this point, if I interpret Dr. Merz rightly, he suggests that Science and Religion are conceived of as opposed interests just because it is forgotten that the outer world, of which Science treats, is only a part, and a very small part, of the whole field of consciousness which forms the larger and wider totality of all our experiences. Religion is concerned with the whole, of which the physical world is but a selected portion. Thus both aspects of the world, the scientific and the religious, spring from the common-sense view which we unconsciously adopt in the early years of life.

Part II. is occupied with showing in outline how the scientific view has arisen and has been developed through long elaboration. Science, dealing with a restricted and clearly defined portion of the total field of consciousness, carries still further the differentiation effected by common sense between the public world of experiences we share, or think we share, with others, and the private larger portion of the conscious field which forms a possession of each separate self. This selection is rendered more precise by scientific methods of measurement, explanatory conceptions like cause or substance, and so forth. "But on this road of description and explanation," says Dr. Merz, "we go a step further, and desire to have an interpretation of the external world or the Universe in its relation to our own selves which are included in it. In doing so we transcend the limits of external observation and bring in imperceptibly a reference to our personal feelings, desires, and volitions." A special study is required to deal with the aspect of things indicated by such words as end or purpose. This study is the subject of Part III., entitled Religion.

Dr. Merz calls this the "subjective" interpretation of the Universe, but he fails to justify the epithet, more particularly in a psychological point of view. At any rate, in this last section we are shown Religion endeavouring to recover the lost unity of experience—a function which Art and Poetry, though unsuccessfully, also strive to discharge. The satisfaction derivable from Art is open only to a comparatively small number of persons, and is likewise a fleeting thing, too much detached from daily life. We want such an interpretation of inner experience as will bring our emotions into some kind of intelligible connexion with the realities that surround us, will contain a rule of conduct fruitful in inner satisfaction, and will have a definitely social reference. "The pressure which we feel in the inner world indicates a reality which embraces the whole field of our consciousness, and which, if we could see it as clearly as we do restricted portions of the outer world, would explain to us and interpret the whole of reality" (p. 145). This reality behind our emotional life is most appropriately termed "Spirit". To define Spirit and point out more clearly the relation in which we stand to it, is the principal task of the religious view of the world. Doubtless religion has changed its doctrines in the course of history, but Science, to be fair, has done so too. Indeed, "the ultimate problem of life is in fact always the same, whereas the problems of science are continually changing" (p. 149). And the spiritual view, although vague, may yet be certain in content and meaning. Truth in this deepest sense is not attainable through critical analysis and dissection: these matters cannot be communicated through words; rather, as Plato says, "out of repeated debates on them, and much social intercourse, there is kindled suddenly a light in the mind, as from fire bursting forth, which, when once generated, keeps itself alive". To repeat once more

the main thesis of the Essay—the inner (spiritual and emotional) world is not opposed to the outer (sensuous and intellectual), but embraces it as the larger field of thought. Hence it is in Religion that at length we find again, at a higher plane, that synoptic view of experience with which life began.

I do not propose to remark upon the genetic account which Dr. Merz has offered of how our full-grown view of the world has been developed, or of the selective processes of Science, working first by way of quantitative measurement, then of abstract conceptions or categories. But one or two observations may be devoted to the book as a whole, read as a descriptive psychological account of the genesis of the religious interpretation of experience. To begin with, I should urge that psychology is not really capable of interpreting religion adequately. You cannot say with precision what religion is if you leave out the interest of the religious mind in truth, in the objective validity of belief; no explanation is complete even psychologically which omits this. Dr. Merz has left this question untouched. But the interest in truth is for religion, a constitutive interest and the psychology of the religious attitude has left its work half-done unless it has elicited the convictions with which the ideas of the believer are laden, and the motives inspiring them. There is not a historical religion which does not tackle this problem of truth in some fashion, with the intention of solving it, and this *intention* is a cardinal datum for the psychologist, whose business is to discover its significance for religious life. Moreover, just as no one who rejects determinism will admit that psychology could possibly give an exhaustive account of an act of moral choice, so, if you hold that faith in God is an illustration, or rather the supreme and culminating instance, of morally free action, you cannot possibly be content with what psychology as such has to say about faith. Psychology must eventually try to explain faith by resolving it into non-religious elements, which is to explain it away. The explanation of the psychologist—an abstraction after all—has of course much indirect value. To a large extent it will enable us to understand the peculiar psychical character of the religious experience, to feel ourselves into the position of an opponent, and to appreciate the part played by religion in spiritual life as a whole, what needs it satisfies, what effects it produces. But this is far from going all the way.

Once more, every psychological report upon religion "breaks bounds" at some point or other. Problems of truth and value intrude where they have no right to be. Thus on p. 176 Dr. Merz writes: "The soul and mind of the Universe—the Divine Spirit—is in this way inevitably endowed in our estimation with the attribute of personality". If we take the word "inevitably" in a psychological sense, however, the statement is obviously inaccurate, since such people as atheists exist. If we take it otherwise, the "inevitability" must be of a logical or ethical kind, and we have

ceased to be psychologists. Other passages—*e.g.* “a higher interpretation sees in them the working of a highest Spiritual Power” (p. 190)—contain the same error. These involuntary divagations into what may be called the theory of religious knowledge appear to prove that after all psychology scarcely touches the problem of “the essence of religion”. That problem is ignored as long as we fail to discuss the question whether religious certainty is valid, whether it rests, as really as scientific or ethical certainty, on necessities of a trans-subjective kind. The fact is, Dr. Merz is throughout not dealing so much with real religion as with the possibility of religion in the life of the soul, which is a quite different matter. Inevitable confusion arises if we do not keep psychology to its proper work of description and classification, while calling in some other interpretative discipline to treat of problems relating to the truth and value of religious experience.

Secondly, by confining himself to introspection and ignoring the inductive or comparative study of religious phenomena Dr. Merz seems to me to give a seriously defective account of how religion actually grows and lives amongst men. He omits the vital bond between genuine religion and history. From his exposition we might suppose that the most important relation in which the believer stands is his relation to nature, rather than his relation to that specific kind of reality we call history—the kind of reality, indeed, in which as a matter of fact we live the distinctively human life. Nature, as far as we have to do with it, resembles the ocean; it has its tides—the seasons—but it makes no progress which is morally significant. History does. And the vast bulk of the most characteristic religious life now upon the planet has been generated not by the contemplation of nature but by the moral infection that pervades history, and is concentrated in facts in which the believer sees revelation. When Dr. Merz speaks of the Ideal or Spiritual aspect of things, he means a higher interpretation of the same reality with which physical science deals, not (at least predominantly) the interpretation of a reality quite different in type. With this agrees the fact that he says little or nothing of the living bond uniting faith to moral experience. For morality too is unintelligible apart from the conception of history, of progress.

History, as a distinctive sort of reality, has come off rather badly at the hands of philosophers. We are still under the spell of the Greek tradition, for which, as for the Indian, reality in the deepest sense of the word is simply immutable. When it was urged that room must be found for a different type of real being, conceivably a higher type, people sought to discount the claim by reducing historical development to terms capable of being covered by natural law. Others exhibited (and discredited) the historical movement as finding its τέλος in the actual present. Others, seizing on a function which Kant had reserved for the *intellectus archetypus*, professed to set out the entire plan of history as a moving dialectic.

In every case the fact was ignored that it is *individuals* who have created the substantial fabric of the past, and that ideas have acted in life only through individuals whose ideas they were. Nothing has so badgered absolute systems of philosophy as the irritating fact of personality—unique, unrepeated, non-transferable. But in recent years men like Windelband and Rickert have at least made a beginning with the better view that history is different from nature because it is the sphere of the true individual. What the science of history has to do is to interpret this individual, who makes a contribution to human life that no general categories can ever fitly represent. All individuals have made some contribution, and many believe that the Founder of Christianity made a contribution which opened a new era. Certain supreme values were then realised for the first time.

Now to ignore this aspect of things, when we are trying to understand how men come to be religious, is, I should plead, studiously to miss the point. Apart from historical experience, religion is only a word; it is only in history that those societies are born without which faith could neither live nor be propagated. Furthermore, once we appreciate the crucial importance of the historical mediation for religious life, we perceive why the question of truth is vital. The mystic, indifferent to history, is equally indifferent to the trans-subjective significance of his moods. Genuine religion is never evoked apart from personal influence as creative of individual conviction and experience, and this comes to men through historical media and only so. While mysticism, at all events in its characteristic Eastern forms, does not lead the worshipper to identify himself with any purpose of God in the world, religion takes possession of the world in God's name; and it does so because, taught by history and its ethical meanings, it has come to regard the contents of the human time-series as the vesture or embodiment of a steadfast and prevailing aim. Hence to believe is not so much to have attained to a certain view of nature as to have perceived in the developing human story the presence of an ethical Power before whom we bow in unconditional surrender.

H. R. MACKINTOSH.

Senescence and Rejuvenescence. By CHARLES MANNING CHILD, of the Department of Zoology, the University of Chicago. The University of Chicago Press, Chicago, Illinois, 1915. Pp. 465. (Agents for United Kingdom: Cambridge University Press.)

THIS book is a worthy presentation of fifteen years of research. It is comprehensive, thorough, lucid, abundantly documented and adequately indexed. The facts are carefully ordered, analysed and discussed. The theories emerge out of them as reasoned hypo-

theses. The argument against other hypotheses, if not always quite convincing, is always fair and well grounded. Like some other studies issued by the University of Chicago Press, whose agents in the United Kingdom are the Cambridge University Press, this book must be reckoned with in any future studies along the same lines. And these lines lead to all the great hypotheses of biology.

These general phrases convey no adequate conception of the wealth of factual materials adduced and discussed. In any case this would not be the place for the technical analysis of the experimental results. It is enough to indicate a few of the main issues. The problems are discussed from various standpoints in five parts—the problem of organic constitution, an experimental study of physiological senescence and rejuvenescence in the lower animals, individuation and reproduction in relation to the age cycle, gametic reproduction in relation to the age cycle, theoretical and critical discussion of results and theories. As the outcome of his facts and observations, Mr. Child constructs the following view of the nature of senescence: "Senescence is primarily a decrease in rate of dynamic processes conditioned by the accumulation, differentiation, and other associated changes of the colloid substratum. Rejuvenescence is an increase in rate of dynamic processes conditioned by the changes in the colloid substratum in reduction and dedifferentiation. Senescence is then a necessary and inevitable feature of growth and differentiation" (p. 59). "Early embryonic development is essentially a period of dedifferentiation and rejuvenescence" (*ib.*). This condensed statement contains many difficult points; but these points the book has been written to discuss. Of colloids, Mr. Child says—"In fact, the more we know concerning colloids the less possible it becomes to conceive of anything similar to what we regard as life without them. Whatever else it may be, it seems certain that the organism is a colloid system. From this point of view, our definition of the living organism must be somewhat as follows: a living organism is a specific complex of dynamic changes occurring in a specific colloid substratum, which is itself a product of such changes, which influences their course and character and is altered by them" (p. 26). This, so far as it goes, is a positive definition of organism in terms of chemistry, with the important qualification "whatever else it may be". Given the colloid substratum, the chemical reactions that proceed within it result in the accumulation of materials and in the slowing of the rate of metabolism. This is the process of growth, which is essentially the same as senescence, whose "limit" is death. Senescence is thus a normal and universal phenomenon in the progressive development of the organism.

To establish this generalisation, Mr. Child has first to dispose of conflicting theories. This he essays to do provisionally at the opening of the book. With him, individuation, differentiation, de-

differentiation (that is reversion to embryonic conditions), senescence, rejuvenescence, inheritance, inheritability, evolution, are all "moments" in the age cycle of the organism. In every grade of organism, from the lower plants to the higher animals, senescence, in one tissue or another, and in one degree or another, alternates with rejuvenescence, differentiation with dedifferentiation. Reproduction, whether sexual or vegetative, whether it be reproduction of the whole organism or reproduction of one cell from another, is fundamentally the same process, being a mode of rejuvenescence (dedifferentiation). This generalisation, if it can be established, negatives the "corpuscular theories" of reproduction, for example, Weismann's germ plasm (p. 461), which presupposes that the reproductive cell is differentiated from the beginning and continuously reproduces itself, the somatic cell being, as it were, a by-product. Mr. Child, on the contrary, maintains that "germ plasm is any protoplasm capable, under the proper conditions, of undergoing regression, rejuvenescence and reconstitution, into a new individual, organism, or part. In other words, germ plasm becomes merely an abstract idea which connotes the sum total of the inherent capacities or 'potencies' with which a reproductive element of any kind, natural or artificial, agamic or gametic, giving rise to a whole or a part, enters upon the developmental process. Germ plasm then is merely another term for heredity" (p. 462). This apparently dogmatic rendering is supported by a number of particular and general arguments, some of which, however, the Weismannist can readily meet. Further, Mr. Child maintains that neovitalism, including Driesch's theory of entelechy, is necessitated by such corpuscular theories as Weismann's; for each unit element of the germ plasm is imagined as itself an organism, capable of growth and relatively independent, and to create a major organism out of these elements needs some "principle," which Driesch provides in his entelechy. Mr. Child denies the validity of the corpuscular theories and considers that "to-day there is less basis for vitalistic theories than ever before" (p. 10). He admits, however, that Driesch is right in requiring some "principle" capable of establishing domination and subordination within the organism. But this principle Mr. Child finds, not in entelechy or any such hypothesis, but in the difference between the rates of metabolism in different portions of the organism (p. 54). This is sufficient to establish the "orderly course and definite result of differentiation" (p. 54), without assuming any entelechy. In plants, an illustration of this is the dominance of the vegetative tip over other parts. Such a dominance must exist equally in animals, but it is more difficult to establish. His experiments, however, with planarian worms and the delicate susceptibility tests applied to establish differences in the rate of metabolism between a "head" and other sections, do show differences in the rate of metabolism, and establish the fact that, in the tissues in which the rate of metabolism is

highest, stability is greatest. On this fact the whole theory of development rests. But in the wealth of inductive detail, I cannot trace any effort to explain why differences of metabolism should establish themselves in the ways they do. Nor do I feel that Mr. Child does full justice to the body of observations that suggested the continuity of the relatively segregated germ plasm of Galton and Weismann, or the mutations of De Vries, or the inheritable unit elements of Mendel. Of his arguments against the corpuscular theories and inferentially against the need for "vitalism," it seems to me that the strongest are (a) that the assumed units merely repeat the problem of the major organism, and (b) that if there is continuity from lifeless matter to living matter, the process is either all "mechanism" or all "purpose". He does not put the alternative in this precise way, but his theory implies that there is "a firm foundation for the belief that the living must have arisen from the lifeless and that the fundamental laws governing both are the same" (p. 13). In later parts of the book, he rejects the proximate teleology of the organism, but admits a possible teleological view of the world as a whole—a doubtfully coherent position.

There is no space to discuss these perennials of progressive controversy; I must take leave of this admirable volume of positive research with one or two excerpts: "Some degree of rejuvenescence occurs even in man, and different tissues differ as regards their capacity for rejuvenescence, the central nervous system being apparently least capable of regressive changes. This characteristic of the nervous system suggests the probability that the natural or physiological length of life in these forms is determined primarily by the length of life of the nervous system and that physiological death is primarily the death, as the final stage of senescence, of the nervous system. . . . For his high degree of individuation man pays the penalty of individual death, and the conditions and processes in the human organism which lead to death in the end are the conditions and processes which make man what he is. The advance of knowledge and of experimental technique may make it possible at some future time to bring about a greater degree of rejuvenescence and retardation of senescence in man and the higher animals than is now possible. But when we remember that the present condition of the protoplasmic substratum of these organisms is the result of millions of years of evolutionary equilibration, we cannot but admit that this task may prove to be one of considerable difficulty" (p. 310).

"The nervous system undergoes the least reduction in starvation" (p. 44). It is the most stable. "During the earlier stages of development, it certainly has the highest metabolic rate of any part of the body and in many cases, if not in all, this condition persists throughout life. Furthermore, during the later stages of life its special functional activity is certainly almost, if not quite, continuous" (p. 44).

"The cells of the nervous system in man and many animals are believed to persist throughout life, and to possess no appreciable capacity for regression and dedifferentiation beyond their ability to regenerate the nerve fibres which arise from them. Doubtless this belief is correct, so far as visible structural changes or measurable metabolic changes are concerned; but is there not reason to believe that the effect of a change in mental occupation or of a vacation after long-continued mental labour in a particular field is in some slight degree a rejuvenescence of the nerve cells? Many facts indicate that a reasonable variety in mental occupation is a factor in retarding mental senility" (p. 296).

"If we admit that the gametes are integral parts of the organism, there is no theoretical difficulty in the way of such inheritance (of acquired characters). Whatever the theoretical possibilities may be, it is, in my opinion, quite impossible to account for the course of evolution and particularly for many so-called adaptations in organisms without the inheritance of such acquired characters, but since thousands or tens of thousands of generations may be necessary in many cases for inheritance of this kind to become appreciable, it is not strange that experimental evidence upon this point is still conflicting" (p. 463). This seems to me open to the criticism that Mr. Child relevantly makes of the corpuscular theories—"these theories do not help us in any way to solve any of the fundamental problems of biology; they merely serve to place these problems beyond the reach of scientific investigation" (p. 11). Undoubtedly, Mr. Child adduces many facts in support of his far-reaching generalisation, but his views on the specialised differentiation legitimately named "inheritance" certainly need further exposition. In this book, his views are rather a formulation than a solution of the problem. When he says that "if evolution is in some degree a secular differentiation and senescence of protoplasm, the possibility of evolutionary rejuvenescence must not be overlooked" (p. 464), he is asking his generalisation to do merely fanciful service. In this volume he practically promises another book and the world of biology will look forward to it with keen interest.

W. LESLIE MACKENZIE.

VII.—NEW BOOKS.

*Group Theories of Religion and the Individual.*¹ By CLEMENT C. J. WEBB.

GREAT interest and I think a high value attaches to this volume of lectures, in which an eager and distinguished student of religious philosophy criticises the religious theory of the group of French scholars whose organ is *L'Année Sociologique*. Subject to reservations which the author makes with care and precision in the case of M. Durkheim himself, their view (notably that of M. Lévy Bruhl) is that religion belongs to a primitive and "prelogical" stage of human thought, and is destined to vanish in proportion as the reason of the civilised individual obtains its due predominance. For religion is rooted in collective representations ; and collective representations² are always more or less illusory, as treated by the dry light of the individual intelligence.

Mr. Webb has no difficulty in showing that M. Lévy Bruhl's conception of a prelogical human mentality, which has no law of contradiction, but has a "law of participation" incompatible with our thought, is a mere figment, depending upon misunderstanding of simple differences in degree of enlightenment. Soo too with the idea that "the logical hierarchy is but an aspect of the social hierarchy". Classification is not explained by the fact that early classifications are affected by social analogies. With M. Durkheim the case is a little different. Though his definition of religion seems quite narrow and untenable, yet he recognises a true reality as appearing in the categories and religious phenomena which exhibit themselves first in social form. But on the whole, Mr. Webb finds that the group-theories, while recognising what are important aspects of religion in the identity of the deity with the group members, are altogether inadequate both as suggesting that religion is a vanishing survival of a prelogical age, and particularly in failing to deal with the individual experience of it. A dogmatic individualism, he thinks, frustrates in them every attempt to appreciate the individual's self-transcendence, and his need for an individual response in the Deity.

Thus he is less sympathetic than perhaps might have been expected towards even Miss Harrison's and Mr. Cornford's ideas ; and finds the recognition of the individual's religious experience deficient, as throughout the group-theories, so in some which aspire to be more than group-theories. As Mr. Webb holds out a hope of what would be most welcome, a further investigation into these latter suggestions, I will venture, in order to make clear what he has to meet, the remark that in his concluding pages he seems to me to adhere too closely to an old conception

¹ *Group Theories of Religion and the Individual*, by Clement C. J. Webb, Fellow of Magdalen College, Oxford, late Wilde Lecturer on Natural and Comparative Religion in the University of Oxford. London, George Allen & Unwin, 1916. Pp. 205.

² Mr. Webb has a charge to bring in respect of an encouragement of such phrases as "collective representations" against many modern psychologists and logicians, in sympathy with Mr. Joseph's remarks, *MIND*, xix., 76, p. 468. It is odd that Green uses the phrase, *Works*, ii., 287. I do not know its history.

in taking the individual to be concrete and the universal to be abstract. In any ultimate account, I should have said, the individual of which he is speaking—the given person or thing—must be abstract, and the universal must be concrete.

I have been unable in this short space to express how greatly I value this book, and how absolutely successful it appears to me in its primary contention.

BERNARD BOSANQUET.

Social Adaptation. A Study in the Development of the Doctrine of Adaptation as a Theory of Social Progress. By LUCIUS MOODY BRISTOL, Ph.D., Assistant Professor of Sociology in West Virginia University, with a Preface by Thomas Nixon Carver. Cambridge: Harvard University Press, 1915. Pp. xii, 356. Volume xiv. of "Harvard Economic Studies".

This is a treatise which was awarded the David A. Wells prize for the year 1914-15, and is published under the direction of the Department of Economics of Harvard University. It consists of a successive treatment of the views of modern writers which bear upon social adaptation, beginning with Auguste Comte and ending with William James and Mr. E. A. Ross. The framework is a distinction of adaptation into passive, i.e., non-purposeful, material and spiritual, and again active, i.e., purposeful, material and spiritual. Thus adaptation is carried beyond its biological meaning, and comes to include everything that can be done for social and individual progress.

Professor Carver's preface prepares us for a doctrine of social evolution which will have a revolutionary effect by demanding a morality in accordance with the order of nature and of the universe—a morality of natural selection, of strength and efficiency. But nature perhaps is one thing, and the universe is another. We seem to have heard such phrases before, and we have observed that nature in the strict sense is not all there is in the universe; while if it is not to be in the strict sense, our standard is still to seek.

The author of the treatise is a good deal more discreet than the preface writer. He is aware that the weak may convert the strong, and that the effect of example is one of the noblest modes of prevalence—was it necessary to crystallise this truth in such a word as "exemplification"? But in the end, after going through a miniature history of sociology, out of which there is developed a view to be called "social-personalism," we come back to this, "All these unities and all forms of associational life are means to the attainment of the one supreme good—the well-being of the greatest number of rational individuals including not only the present but future generations". To make this anything like true, must we not parody the Benthamite ambiguity, and say "the highest well-being of the greatest number of rational individuals"? This would leave the main question undecided, but at least not falsely closed.

Something is wanting, which might have been learned for instance from Nietzsche, of the imperativeness of great achievements and purposes. But when we turn to the account of Nietzsche we see that it is largely second-hand, and adopts a commonplace point of view. This is the Nemesis of these very inclusive studies; the accounts of individual writers are not very valuable, while the main thesis of the book is insufficiently worked out. I do not mean that the treatise is other than sensible and instructive.

BERNARD BOSANQUET.

Psychology in Daily Life. By CARL EMIL SEASHORE, Professor of Psychology and Dean of the Graduate College in the State University of Ohio. Conduct of the Mind Series, edited by Joseph Jastrow. London and New York : D. Appleton & Co., 1914. Pp. xviii, 225.

"It is," says Prof. Jastrow in his general introduction, "the purpose of the series to provide readily intelligible surveys of selected aspects of the study of mind and of its applications." Again he says, "the desire to apply this knowledge reflects the stress of the practical temper; the need of adaptation of the mental equipment to the complex conditions of modern life is insistent". The motive of the present series is "to supply the foundation in principle for the guidance of practice". In a special introduction to Prof. Seashore's book, Prof. Jastrow more closely justifies the present volume, which includes chapters on play, serviceable memory, mental efficiency, mental health, mental law, law in illusion, mental measurement. "The spirit of play makes the game of life; the skill in exercising it makes the artist. . . . The problem of the conduct of mind is presented as the regulation of work and play" (p. xi). Prof. Seashore is lucid in expression, concise, directly practical, and vivid. The presentation, though popular in language and form, everywhere rests on the verified results of the psychological laboratory. Here and there, the directive rules of action and the stress laid on efficiency suggest the over-seriousness of a people "on the make"; but the rules are relevant and practical and every civilisation needs them. The vocational analysis of a singer is an excellent illustration of the methods of studying the individual. The book aims at giving a psychological perspective to conduct and in this it succeeds; it is rather a book for practice than for summary.

W. L. M.

Human Motives. By JAMES JACKSON PUTNAM, M.D., Professor Emeritus, Diseases of the Nervous System, Harvard University. London : William Heinemann, 1915. Pp. 175.

The Meaning of Dreams. By ISADOR H. CORIAT, M.D., First Assistant Visiting Physician for Diseases of the Nervous System, Boston City Hospital ; Instructor in Neurology, Tufts College Medical School. London : William Heinemann, 1915. Pp. 191.

Sleep and Sleeplessness. By H. ADDINGTON BRUCE, A.M., Author of *Scientific Mental Healing*, *The Riddle of Personality*, etc. London : William Heinemann, 1915. Pp. 215.

These three volumes are items in a Mind and Health Series. Each volume is written by a man familiar with his material. In general, all three volumes may be regarded as illustrations of the Freud psychology, not accepted uncritically, but kept throughout as the main standpoint.

In *Human Motives*, Dr. Putnam sketches the main sources of motives, the rational basis of religion, the psycho-analytic movement and its bearings on education, the relation of instincts and ideals, and the possibility of bringing them into synthesis. His philosophical standpoint in the chapter on the "Rational Basis of Religion" is a little difficult to grasp; but it is essentially an effort to express the notion underlying Bergson's *élan vital*, self-creative energy. On the practical side, especially in his account of the psycho-analytic movement and its bearings on education, he shows the wise sense that comes only from practical insight and experience. He explains how the apparent or asserted motive may be a mask for a "repressed" emotion. In education,

"too much stress is often laid upon the act, and too little upon the tendency of which the particular act may be a sign" (p. 113). Individualism, when it is not a stepping-stone to something better, "unmasks itself as a sign that the development of the individual received a check at an earlier stage, and that we have before us a situation of immaturity" (p. 160). This small book is full of penetrative remarks of this order. It forms an admirable introduction to the series.

In *The Meaning of Dreams*, Dr. Coriat gives a simple and admirably balanced account of the Freud theory of dreams and its applications. He deals with the nature of dreams; the method of dream analysis; dreams as the fulfilment of wishes; dreams and the unconscious, and the mechanism of dreams as now made familiar in many recent volumes and discussions. This volume forms a good general introduction to the fundamental ideas of psycho-analysis.

In *Sleep and Sleeplessness*, Dr. Addington Bruce, Editor of the Series, gives full value to the Freudian standpoint, but is not bound by it. He deals with the whole question of sleep and sleeplessness in an eminently practical way. "Almost always, in fine, there is hope for the insomniac; almost always his is distinctly a curable malady; and almost always, in the last analysis, the cure rests with himself" (p. 215).

These three well-written and well-printed volumes are good illustrations of applied psychology. It is natural that, for the moment, the Freudian standpoint should predominate; but it is right that the great practical value of Freud's methods should become widely enough known to provoke the necessary qualifying criticism.

W. L. M.

A Historical Introduction to Ethics. By THOMAS VERNER MOORE, Ph.D.
New York: American Book Co., 1915. Pp. viii, 164.

As the author states in his preface, this book is not meant to be a history or a text-book of Ethics, but an introductory treatment of leading ethical doctrines, illustrated by reference to prominent names in the history of Ethics. The standpoint of the book may be best indicated by saying at the outset that it appears to have been written especially for students in Roman Catholic seminaries. In the first part of the book the different forms of Hedonism are considered under the designation, "Conditionate Morality"; the second part discusses other systems under the name of "Absolute Morality"; and the third part is devoted to criticism. The separation of the criticism from the account of a writer leads to some repetition, but in the early stages of such study that is an advantage. The book is, however, very uneven. Butler and Green are not mentioned, and only five paragraphs, making a page and a half, are given to Sidgwick, while Rousseau, who there "can be no doubt" was "psychopathic," receives five pages. The use of the term "Egoistic Utilitarianism" is to be deprecated, especially as "Egoistic Hedonism" and "Utilitarianism" are generally accepted as quite distinct. The ethical doctrine of Kant assumes a different appearance according to the place which one gives to the concept of "humanity in thine own person" as an end: Dr. Moore merely mentions the form of the Categorical Imperative in which this occurs. The conception of a "realm of ends" is not referred to at all, though it is a valuable one for the standpoint of the book. Not only is there no consideration of the different attitudes which Plato assumed towards Hedonism, but also no hint is given of the fundamental reason for its rejection, that the good must be enduring, while pleasure is transient. In the comparatively long account of

Aristotle no reference is made to the idea of the "mean" relative to the individual in accordance with right reason, and as the wise, practical man would decide. The account of the ethical system of St. Thomas Aquinas is the longest, and as one might reasonably expect, is really good. A good feature of the discussions is that the positions of the authors are very frequently stated in their own words.

In an Introduction contributed by Dr. Shahān there is a misunderstanding of the position of those who would make Ethics a "positive" science. As a "positive" science Ethics would be a systematisation and consideration of moral judgments and the hypothetical elaboration of the ideal standards they imply. The application of these to the test of any particular act of conduct, intention, or character will be normative, but such application is beyond the scope of the science of Ethics. Ethics as a science of "moral facts" is simply Ethics as a science of "moral judgments," and the indefinable goods they refer to. Such a positive science may even admit that "a full and perfect ethics" is not possible without considering the relation of man and God. If these "indefinable goods" are admitted—as they must be—the conclusion is forced upon one that the "moral judgment" is not merely "rational" (even mathematical judgments are not), and, in opposition to Dr. Moore, we must say that "Reason" may not be "the ultimate channel through which we receive the knowledge of right and wrong" (143), essential though it be in the making of moral judgments.

The author does not seem to recognise that though to give "numerical" values to "emotional states," or to sum such state "numerically" is impossible, in reflection and deliberation before moral choice we do in fact compare all the foreseen consequences of the different possible modes of conduct, and do in some sense contrast one sum of values with others. And this is the case not only for the individual but also for society, in so far as men deliberate on the issues before they exercise their political power.

There are useful lists of books for reference and further study of the various authors discussed, but no list of books of systematic Ethics. Yet the students of this book would do well to study a few books like Mackenzie's *Manual of Ethics*, Rickaby's *Moral Philosophy*, Seth's *Ethical Principles*, or Rashdall's *Theory of Good and Evil*, along with the books mentioned.

ALBAN G. WIDGERY.

Deliverance—The Freeing of the Spirit in the Ancient World. By HENRY OSBORN TAYLOR, Litt.D. Macmillan & Co., 1915. Cr. 8vo. Pp. 298. 6s.

Dr. Taylor's small volume is worthy of the attention of the general reading public, not only for the real interest of the subject-matter but also for its pleasing style. It is a survey of the chief attempts of the ancient world to find satisfaction for life's needs. These attempts are associated by Dr. Taylor with the great personalities of the past, starting with Hammurabi, and passing on to Confucius, Lao Tzu, Gotama, Zarathustra, the Prophets of Israel, the Poets and Philosophers of Greece, the Roman Stoics, Jesus, Paul, and Augustine. Throughout, one fact at least becomes clear, and should be taken to heart by those students of religions who emphasise their similarities at the expense of their differences: "the needs of men are not the same universally". Even where the conclusions arrived at are similar, as e.g. in the Buddhist conception of the impermanence of all component things, and in the theory of *l'aux*

of Herakleitos, the motives which led to them were not the same. It would be more correct if Dr. Taylor had said that men have not always felt the same needs with the same intensity. The differences are largely differences of emphasis. The problem of life is for all one of "adjustment"—with reference to what is "within" and to what is "without" the personal individual. The one universal element is the "love of the best" which drives man on to perfect peace and perfect freedom. The endeavour itself seems to be the attainment, the path the goal. Though much in the volume is very vague the author succeeds remarkably well in extracting what is fundamental in the different attempts and in presenting it in carefully chosen words and with many apt quotations. It is to be regretted that the references to the quotations are so inadequate. The main defects of the "adjustments" are indicated, as e.g. that the Indian attempts are not merely careless of the individual but even seek adjustment itself in its elimination.

The short account of Gotama is particularly good, and that of Jesus equally poor. It is of no value to say we must treat Jesus as "the Messiah and the Son of God—as he deemed himself," unless we say clearly what is to be understood by these terms. The nature of the "adjustment" associated with the name of Jesus depends on the interpretation we give to these words. The account of Greek attempts ought to have led on to Plotinus and Neo-platonism, and not have ended with the discussion of Epicureanism. Again, seeing that the leading minds of all the great religions except Islâm are considered, it is a pity Dr. Taylor did not extend his treatment far enough to take in Mahomet.

It would take far too much time and space to examine the details of this thoughtful volume. Suffice it to say that the chief defect of the conclusion of the book is the failure to make sufficiently clear that these attempts are mostly one-sided. Thus, though it may be to some extent true with reference to Stoicism that "As a means of human adjustment and of deliverance, philosophic thought has broken down," it must be insisted that philosophic thought is none the less *one* of the means of adjustment and of deliverance for minds with intellectual needs. On the last page the author expresses doubt concerning individual immortality, and finally says, "Less rapturous, more analytic, tempers may also conclude that only infinite life is suited to eternity, not man but God". The question may indeed be asked whether after all that is not what the best thought of Hinduism has always meant in that endeavour to eliminate individuality, for which Dr. Taylor has himself criticised it.

ALBAN G. WIDGERY.

Received also :—

- Eric S. Robertson, *The Bible's Prose Epic of Eve and Her Sons*, London, Williams & Norgate, 1916, pp. vii, 291.
 Maurice Maeterlinck, *The Wrack of the Storm*, London, Methuen & Co., 1916, pp. x, 277.
 G. Van Ness Dearborn, *The Influence of Joy*, London, Heinemann, 1916, pp. xviii, 223.
 Herbert Branston Gray and Samuel Turner, *Eclipse or Empire?* London, Nisbet & Co., pp. x, 316.
 Florian Cajori, *William Oughtred, A Great Seventeenth-Century Teacher of Mathematics*, Chicago, London, The Open Court Publishing Co., 1916, pp. vi, 100.
 Rev. J. O. Bevan, *Handbook of the History and Development of Philosophy*, London, Chapman & Hall, 1916, pp. viii, 223.

- Antony J. Philpott, *The Quest for Dean Bridgman Conner*, London, Heinemann, 1915, pp. viii, 251.
- Henry Maudsley, *Organic to Human: Psychological and Sociological*, London, Macmillan & Co., Ltd., 1916, pp. viii, 386.
- G. Santayana, late Professor of Philosophy in Harvard University, *Egotism in German Philosophy*, London and Toronto, pp. 171.
- Sir Bampfylde Fuller, *Man as He is*, Essays in a New Psychology, London, John Murray, 1916, pp. 247.
- John Dewey, *Essays in Experimental Logic*, Chicago, The University of Chicago Press, 1916, pp. vi, 444.
- F. Otto Schrader, *Introduction to the Pancaratra and the Ahirbudhnya Samhita*, Madras, Asyar Library, 1916, pp. xi, 177.
- Twenty-ninth Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution, 1907-1908*, Washington, Government Printing Office, 1916, pp. 636.
- Thirtieth Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution, 1908-1909*, Washington, Government Printing Office, 1915, pp. 453.
- Benchara Branford, *Janus and Vesta, a Study of the World Crisis and After*, London, Chatto & Windus, 1916, pp. xviii, 316.
- C. A. Wynschenk Dom, translated from the Flemish, *John of Ruysbroeck, The Adornment of the Spiritual Marriage, The Sparkling Stone, The Book of Supreme Truth*, Edited with an introduction and notes by Evelyn Underhill, London, J. M. Dent & Sons, Ltd., 1916, pp. xxxii, 259.
- H. L. A. Visser, *De Collectieve Psyche in Recht en Staat*, Haarlem, H. D. Tjeenk Willink & Zoon, 1916, pp. viii, 250.
- Jacob Israël de Haan, *Rechtskundige Significa en Hare Toepassing op de Begrippen "Aansprakelijk, Verantwoordelijk, Toerekeningsvatbaar"* Academisch Proefschrift, Amsterdam, W. Versluys, 1916, pp. 273.

VIII.—PHILOSOPHICAL PERIODICALS.

PSYCHOLOGICAL REVIEW. Vol. xxiii., No. 4. **J. R. Angell.** ‘A Reconsideration of James’ Theory of Emotion in the Light of Recent Criticisms.’ [James’ theory is not essentially affected by the work of Sherrington and Cannon.] **H. Carr.** ‘Head’s Theory of Cutaneous Sensitivity.’ [A critical review, warning against hasty acceptance.] **H. K. Haeberlin.** ‘The Theoretical Foundations of Wundt’s Folk-Psychology.’ [Wundt arrives by a conceptual process (creative synthesis, psychical actuality, relation of individual to group, unilinear psychogenesis) at a psychological construct, the folk-soul, with which he operates in a purporting science of over-individual syntheses. He involves himself in contradiction by his *a priori* assumption that, in the study of cultural phenomena, the historical and psychological standpoints are methodologically separable.] **A. F. Bronner.** ‘Attitude as it Affects Performance of Tests.’ [Cases illustrating the effect of deceitfulness, recalcitrance, sportiveness, emotive disturbance, nervous excitement, lack of confidence.]—Vol. xxiii., No. 5. **J. Peterson.** ‘The Nature and Probable Origin of Binaural Beats.’ [Binaural beats cannot be referred to bone conduction; they and the perception of phase-differences are cortical in origin.] **J. V. Haberman.** ‘The Intelligence Examination and Evaluation; a Study of the Child’s Mind.’ [Plea for medical co-operation in the administration of tests; detail of tests of knowledge and memory.] **C. E. Ferree and G. Rand.** ‘A Substitute for an Artificial Pupil.’ [The writers use the corneal image of the spectroscope-slit.] **W. F. Dearborn and H. S. Langfeld.** ‘Portable Tachistoscope and Memory Apparatus.’ **H. S. Langfeld.** ‘Portable Self-registering Tapping-board and Counter.’ **H. M. Johnson.** ‘A Note on Ferree and Rand’s Method of Photometry.’ [The Method fails to detect large deviations from proportionality of difference in illumination on the compared fields.] **H. C. McComas.** ‘Extravagances in the Motor Theories of Consciousness.’ [The Motor areas are the less important for consciousness; sense-organs are more complex than muscle and gland; organic changes do not closely correspond with conscious states; pure motor aphasia does not disturb central and sensory processes.] Discussion. **T. L. Kelley.** ‘Further Logical Aspects of the Binet Scale.’ [Otis has failed to utilise the principle of partial correlation.]

PHILOSOPHICAL REVIEW. Vol. xxv., No. 4. **A. Lalande.** ‘Philosophy in France in 1915.’ [Effects of the war: historical studies and studies of *droit*; F. Pillon.] **C. L. Morgan.** ‘Continua and Discontinua.’ [Lays a foundation for theory of knowledge in the perception of uniform continua and orderly discontinua. In reference to the spatial world our perception slides along what we perceive; and through such perceptual travelling we come to know facts and truths.] **D. F. Swenson.** ‘The Anti-Intellectualism of Kierkegaard.’ [Logic, according to Kierkegaard, cannot deal with the contingent, the particular, the factually existent; nor can it provide transition from quality to quality.] **M. W. Kehr.**

'The Doctrine of the Self in St. Augustine and in Descartes.' [Both use the self as the basis of philosophy, relate the problem of evil to the self-doctrine, and base power of choice on consciousness of freedom. They differ by their estimates of the relative importance of theology and philosophy.] *Reviews of Books. Summaries of Articles. Notes.* **G. T. Ladd.** 'A Disclaimer and an Explanation.' [Biographical.]—Vol. xxv., No. 5. **E. H. Hollands.** 'Nature, Reason, and the Limits of State Authority.' [Critique of Hegel's state-absolutism. The political organisation, the state, is to be distinguished from the looser organisation (based on manners and customs, language, culture, ideals) of the nation.] **G. H. Sabine.** 'Liberty and the Social System.' [Discussion of Bosanquet. It is as much the nature of society as of the individual to need reorganisation, and social reorganisation must proceed mainly from the intelligent initiative of individuals.] **W. K. Wright.** 'Conscience as Reason and as Emotion.' [Systematic ethics may reconcile rationality and objectivity (Rashdall, Moore) with the results of evolutionary analysis (McDougall, Westermarek).] **C. W. Doxsee.** 'Hume's Relation to Malebranche.' [The two have a like analysis of causation, a negative account of the knowability of self, and a doctrine of natural judgment or belief.] Discussion. **G. C. Bussey and M. D. Crane.** 'Dr. Bosanquet's Doctrine of Freedom.' *Reviews of Books. Notices of New Books. Summaries of articles. Notes.*

AMERICAN JOURNAL OF PSYCHOLOGY. Vol. xxvii., No. 3. **A. Schinz.** 'The Renewal of French Thought on the Eve of the War.' [Notes the return to Orthodox Catholicism.] **E. G. Boring.** 'The Number of Observations upon which a Limen may be Based.' [No positive rule, such as Fernberger's rule of 50, can be laid down.] **S. S. George.** 'The Gesture of Affirmation among the Arabs.' [Petermann's headshake for Yes is a mistaken observation.] **P. F. Swindle.** 'Positive After-images of Long Duration.' [Experiments on successive colour induction in birds (owl, cockatoo) and man, and on simultaneous in man, show that all colours induce themselves first and their antagonists last. Rules are given for the observation of long positive after images.] **C. E. Ferree** and **G. Rand.** 'A Simple Daylight Photometer.' **M. H. Strong** and **E. K. Strong.** 'The Nature of Recognition Memory and of the Localisation of Recognitions.' [Experiments with words. Recognition arises as awareness of relative ease of nervous discharge : it is measured objectively by reaction-time, subjectively by feeling (familiarity, strangeness). Localisation is perhaps a feeling-estimate of amount of familiarity.] **L. Dooley.** 'Psychoanalytic Studies of Genius.' [Review of work of last decade.] **E. B. Titchener** and **H. P. Weld.** 'Minor Studies from the Psychological Laboratory of Cornell University.' **M. Carnes** and **L. C. Shearer.** 'xxviii. Mechanical *vs.* Manual Stimulation in the Determination of the Cutaneous Two-point Limen.' [For most purposes, careful manual stimulation suffices.] **F. P. Boswell** and **W. S. Foster.** 'xxix. On Memorising with the Intention Permanently to Retain.' [In the case of learning a vocabulary, the intent helps to secure the desired end.] **A. J. Brown.** 'xxx. Some Uses of Artificial Daylight in the Psychological Laboratory.' **G. English.** 'xxxi. On the Psychological Response to Unknown Proper Names.' [No constant tendency is found ; individual differences are very large.] **E. B. Titchener.** 'A Note on the Compensation of Odours.' [Reply to Henning.] Book Reviews. Book Notes.—Vol. xxvii., No. 4. **K. M. Dallenbach.** 'The Measurement of Attention in the Field of Cutaneous Sensation.' [Applies to the skin (intensity and extensibility of faradisation) the method used by Geissler for sight and by the writer for hearing ; similar results.] **H. Clark.** 'Visual

Imagery and Attention: an Analytical Study. [Ocular movements are connected rather with general central and conscious conditions (attention) than with special differences of function of images.] **G. C. and C. E. Myers.** 'Reconstructive Recall.' [Study of recall of prose, verse, names once well known but now almost completely forgotten; pedagogical suggestions.] **H. E. Conard and G. F. Arps.** 'An Experimental Study of Economical Learning.' [In the four fundamental operations of arithmetic pupils should be told to think in terms of result only, and to restrict the audio-motorising mechanism.] **C. A. Ruckmich.** 'New Laboratory Equipment.' [Apparatus and charts.] **E. L. Thorndike.** 'Notes on Practice, Improvability, and the Curve of Work.' [The author finds no evidence of initial spurt in mental work; there is a slight warming-up effect, and a still slighter end-spurt.] **E. B. Titchener and H. P. Weld.** 'Minor Studies from the Psychological Laboratory of Cornell University.' **F. L. Dimmick.** 'xxxii. On Cutaneous After-images.' [Work on pressure spots with an intensity subliminal for subcutaneous pressure.] **E. de Laski.** 'xxxiii. On Perceptive Forms Below the Level of the Two-point Limen.' [Further evidence that subliminal separations of aesthesiometer-points are discriminable.] **W. D. Wallis.** 'Is Introspection Individual or Social, Within or Without?' [Criticism of McDougall; the psychologist must objectify and socialise his mental contents.] Book Reviews. Book Notes.

JOURNAL OF PHILOSOPHY, PSYCHOLOGY, AND SCIENTIFIC METHODS. xiii., 10. **H. G. Hartmann.** 'Science and Epistemology.' [Assuming that 'objective truth' is truth in the production of which the psycho-physical individual is in no way involved, "the writer sets himself to show from science that the aforesaid individual is *not* involved at every point in the determination of reality". To do this he appeals to a chemical experiment, the making of water, in which he says the observer is irrelevant. He does not, however, notice that the notions of relevance and irrelevance are deeply tainted with 'subjectivity,' i.e., relativity to purposes, and that no epistemologist who knew his business would be likely to pass his *ex post facto* appeal to so old and stale an experiment as really illustrative of the procedure of scientific knowing.] **J. L. Perrier.** 'The Permanent Contributions of the Pragmatists.' [Without any attempt at analysing pragmatist doctrine it is decided that the most important are (1) "the temporal character of reality," and (2) "the human element in the building up of reality," and also of 'scientific truths'. It is, however, admitted that the first does not belong exclusively to pragmatism, while the secend would seem more properly to belong to humanism, which is distinguished, by an 'abyss,' from the "original timid principles" of James.]—xiii., 11. **B. H. Bode.** 'Ernst Mach and the New Empiricism.' [A good exposition, which points out, however, that in treating 'sensations' as ultimate 'elements,' Mach failed to carry through the empirical method consistently. Still he saw that this method "meant ultimately that philosophy must justify itself, not as an intellectual pastime or as an emotional indulgence, nor yet as an escape from the unwelcome realities of our present existence, but by its bearing on human weal and woe".] **G. Santayana.** 'Two Rational Moralists [who "have revived the old doctrine that virtue is knowledge". Under this title are reviewed (briefly) Prof. Erskine's *The Moral Obligation to be Intelligent*, and Prof. Holt's *The Freudian Wish* (more fully). The latter is credited with the doctrine that if only a man could fully understand his 'passions' he "cannot go wrong morally," because he will see that he must harmonise them, and also how to do it. San-

tayana objects that knowledge does not "of itself harmonise *ultimate* impulses," or decide *which* of the many ways of harmonising by suppressing incompatibles is the best. Moreover, the difficulty of transferring the principle from an organic body to the world at large is underestimated.] Report on the New York Branch of the American Psychological Association by **A. T. Poffenberger**, jun.—xiii., 12. **H. T. Costello.** 'Professor Macintosh's Pragmatic Realism.' [Gives little information about the book reviewed.] **W. H. Sheldon.** 'The Demolition of Unreality.' [The unreal is to be 'abolished utterly' by declaring that reality "is *either* the same as Being in the most general sense of that term, *or* is a certain sort of Being". But in the latter case it would be Being + a character, and the addition of characters can confer neither Being nor reality. So reality must be "a fulness or acme of Being," and nothing more than "Being in the widest sense". Ergo "unreality means non-being," and is a contradiction. Hence error cannot be 'belief in unreal objects'. "For there are no unreal objects."] **R. B. Owen.** 'The Predicates Real and Unreal.' [Replies, pragmatically, to the last paper that it appears from analysis of the common use of the terms that "reality and unreality are evaluative terms" relative to an interest and a purpose, and the capacity of things of which they are predicated to satisfy or disappoint the same. Hence though unreality in an absolute sense vanishes, because it was a mistake, "things may have the value of being unreal."] **L. J. Henderson.** 'Teleology in Cosmic Evolution: A Reply to Professor Warren.' [Disclaims the latter's account of his position in xiii., 3.]—xiii., 13. **H. C. Brown.** 'Structural Levels in the Scientist's World.' [Scientific explanation, where it does not refer events to a law of which it is an instance, refers them to the lawful behaviour of more elementary facts. These may be called 'levels'. But it should be noted that the behaviour of an aggregate is nowhere the behaviour of its component elements, and that new types of law thus arise which are as significant as the fundamental laws.] **M. T. McClure.** 'Perception and Thinking.' [Attempts to identify the antithesis of perception and thinking with that of mechanism and teleology, and asserts an 'absolute dualism' between "on the one side perception, the particular, unity, mechanism, action; on the other thinking, the universal, multiplicity, teleology, consciousness". It follows that perception is non-cognitive and non-conscious. To the objection that pure sensations are abstractions and do not exist, it is answered that they are this only for knowledge, and that even when sensations occur along with consciousness, the consciousness is no part of the sensation.] **F. L. Wells.** 'Von Bechterew and Uebertragung.' [Expounds 'associative reflexes' and reactions.]—xiii., 14. **H. K. Chadwick.** 'A Suggested Metaphysics to Fit a Functional Epistemology.' [Endeavours to provide Pragmatism (*sp. Deweyensis*) with a metaphysic deduced from the concept of the '*unstatic*,' which "is a chaos of pure motion, activity, kinetic force".] **E. C. Parsons.** 'Primitive Improvidence.' [Suggests that this belief is largely based on 'ethnological ignorance,' and narrates the laborious 'rain-making' of the Zuñis of New Mexico. The providence of savages has merely taken a wrong direction, that of magic.] **H. B. Alexander** and **B. H. Bode.** Report on the Sixteenth Annual Meeting of the Western Philosophical Association, at which political philosophy appears to have predominated, the papers containing much criticism of the Hegelian doctrine of the State.—xiii., 15. **A. K. Rogers.** 'Belief and the Criterion of Truth.' [Starting from the definition that "truth for me is that which I cannot help believing," the author argues that the truth concerned is that of criticised beliefs, and that belief goes deeper than truth and includes more than logical connexion. It includes

acceptance by some one, and implicit faith in man's own nature and instincts. This is the rational basis of practical needs and emotional postulates which affect the reasonings of all, rationalists included.] **E. J. Kempf.** 'Did Consciousness of Self Play Part in the Behaviour of this Monkey?' [Describes the method by which one *Macacus rhesus* robbed another of food.]—xiii., 16. **L. T. Troland.** 'Philosophy and the World's Peace.' [Points out that man is only one of nature's many experiments, of recent origin, and may turn out one of her failures. The question is whether he can master himself and control his emotional instability. The present 'international lunacy of Europe' which has plunged it in "the most beastly war of history" throws a doubt upon this. The truth is that modern society, though systematised in detail, suffered from "a chaos of fundamental purposes. To prevent such a chaos is the function of religion in society." However, Christianity has failed to perform this function, and philosophic ethics also "has been singularly barren of practical results. . . . The consequences for the progress of civilisation, and even of man as a species, may ultimately be very serious. . . . Science alone cannot save us; alone, it may even prove our ruin. What we need is a system of thought, filling the place now occupied by religion, but possessing the strength of science."] **H. W. Wright.** 'The Object of Perception *versus* The Object of Thought.' [Regarding perceptions pragmatically as plans of action, we must yet note that they are also interpretations in terms of past experience functioning as an ideal system. Of such interpretations the 'true' one is selected and verified by objective reality which penalises some ideas when acted on and tolerates others. And as action always means "the motor adjustments of an individual, the object of perception always exists at a particular time and place in an individual experience". The thought-object on the other hand exists as a universal, because "it generalises individual experiences of effort and satisfaction". They are either steps in sequences of movements, and so 'mechanical,' or else sources of satisfaction, and so 'objects of value'. They gain 'objectivity' by claiming to be true and to agree with reality, and are controlled by their conformity with the accepted body of knowledge, and, ultimately, by "the direct verification the ideas of individuals receive in perception and action". The control by accepted knowledge is chiefly exercised through language.] **H. C. Warren.** 'Purpose, Chance, and Other Perplexing Concepts.' [A reply to criticisms by L. J. Henderson, xiii., 12, and J. S. Moore (xiii., 6).]—xiii., 17. **W. M. Urban.** 'Value and Existence.' [Starting from the inconsistencies of philosophic usage, the author tries to reduce values to three types; (1) the adjectival, 'A is valuable,' which forms the ground for the relational theory of value; (2) Value as quality, of the type 'A has value'; (3) the substantive form, 'A is a value'. Moreover, 'value' is used in a narrower sense, relative to a subject, and in a 'broader,' recognising 'absolutely valid,' independent values which are indefinable. It is objected to (1) that in both its forms, the psychological and the ontological, the definition of value is circular—though no attempt is made to show that the circle is vicious, and that the transition from the values which are recognised to those which *ought* to be is not effected by the self-criticism of the former. To (2) the objection is that "it is perfectly possible for an object to have certain (value) qualities and yet as an object to have negative value," though again it seems obvious that both these judgments may merely indicate different phases in the same valuation-process. However it is concluded that the third view must contain the truth. Value must be "an indefinable like existence". But it must not be reduced to existence, because "it adds no new quality to the object". It is an 'objective,' in Meinong's sense, not an object, and is "not a form of being," but "merely

'valid'". This does not account for the usage, recognised in the beginning, which regards 'existence,' 'true reality,' etc., as values, but a further article is promised.] **H. B. Smith.** 'Fact, Definition, and Choice.' [Asks whether there are judgments that "can never be made an experimental issue," and adduces as examples the law of probability, the choice between alternative hypotheses, the adoption of a geometry, and *a priori* judgments, the last of which both absolutist and humanist are supposed to admit to be incapable of empirical verification. But the difference between the empiricist and the absolutist notions of 'verification,' and the methodological use of principles are not examined.]—xiii., 18. **H. G. Hartman.** 'A Revised Conception of Causation and Its Implications.' [Any change is *prima facie* a case of causation, but a cause is never *one* object alone, nor is it *all* objects. It is a number of relevant objects in interaction, which 'originate' the change. What objects interact and what do not is objectively and empirically determinable. To Hume's question—Why should the future resemble the past?—it may be answered that though no universal principle of recurrence may be established, yet recurrence may be observed as a matter of fact, that the supposed future knowledge is congruous with great bodies of past knowledge, and that the postulate of uniformity can be controlled by experience.] **L. H. Miller.** 'A Layman's Question About the "Freudian Wish" as Interpreted by E. B. Holt.' [A catena of quotations, together with the objections they suggest. The point is that Holt has not really banished 'subjective categories,' because they lurk in his unexplained notion of 'integration'.]

REVUE DE MÉTAPHYSIQUE ET DE MORALE. Vol. xxiii., No. 3, May, 1916. **A. N. Whitehead.** 'La théorie relationniste de l'espace.' [The grounds in favour of a relational, as opposed to an absolute, theory of space have been shortly indicated by Russell (*Our Knowledge of the External World*: Chicago and London, 1914, pp. 146, 147) : Whitehead merely says that 'nominally at least, the absolute theory has been almost universally abandoned'. Russell (*ibid.*, pp. vi, 114-115) gives a sketch of Whitehead's definition of a 'point' from the relational point of view, and here Whitehead gives a far more detailed exposition of this definition and other matters. He uses the symbolism of Whitehead and Russell's *Principia Mathematica* for the expression of important definitions, but the rest of the paper, including the indication of demonstrations, is in ordinary language. A distinction is drawn between 'immediate apparent space,' 'complete apparent space,' 'physical space' (the space of physical science, in which molecules and electrons move), and 'abstract space' (the space of geometry). 'The exact analysis of the logical process contained in the parallelism between physical space and complete apparent space, and of the fundamental ideas which have led the human mind to it, do not come into the scope of this paper.' There follows a minute analysis of the axioms which 'often, implicitly or explicitly, govern thought on the subject' of spatial relations between objects and laws of change in these relations. The criticism of the notion of transmission of action by a continuous medium is instructive: action is said by many to be 'transmitted by the contiguous parts of the medium'. The author remarks: 'But there are no contiguous parts in a continuum'. The reviewer would remark that it does not seem necessary for an opponent of action at a distance to speak in the way justly condemned by the author: some people would say that a continuous motion (of a particle along a line) consisted in 'the ceaseless passage from one point to the next'; but still a properly defined continuous motion is logically possible. If it is objected, says Whitehead, that there is no action between material points but only between material

volumes, and that there are contiguous volumes, then : ' Consider the common limit of two contiguous volumes ; the action is produced only across this limit. But there are no infinitely small volumes, so we have two finite volumes acting on one another across their common limit. Now, these two finite volumes may be divided into two parts : no point [position] of either volume which can be included in a volume [also position] not contiguous to the limit contributes to the action, for it is at a distance from the limit. . . . The action is then due to the matter of the points situated on the limit. But this action has been proved impossible.' It seems, however, to the reviewer that, if a volume be a continuum, a point within the volume might act mediately on the point of contact if transmissibility of action were defined (as motion is nowadays) in such a way that it does not assume that there are contiguous points in a continuum. Then the author considers how the objection he mentioned last may be avoided by maintaining that a volume (under a certain magnitude) acts as a whole, and makes use of the important fact that two volumes cannot touch unless one of them is without part of a surface, at least. This leads to what Whitehead calls a very improbable, though logically possible, conception ; but ' the real objection is not this improbability but the unanalysed and uncritical conceptions of space and of objects from which it proceeds. To deny action at a distance is, in fact, to deny direct relations between physical objects which do not occupy the same points ; and that implies the negation of the theory of space-relation.' If the relativist theory of space is to be adopted, it is necessary that points, for example, should be complex entities, logical functions of the relations between objects which make up space. ' The fundamental idea in the relativist theory, in its construction of the concept of a world existing in space, is that of a class of relations (σ). ' Starting from any class (σ) of relations, the author investigates what are the possible definitions of some fundamental spatial concepts and what properties σ must have in order that the usual propositions about the concepts thus defined may be true. A world, thus founded on a class σ , is called a 'world- σ '. Means by which the geometrical concept of 'point' may be defined for a world- σ . The first stage of this definition is the definition of a relation E_σ which is called 'inclusion- σ ', and which is analogous, in its formal properties, to the relation of whole to part. This relation of inclusion- σ may be used to define points. It makes us capable of reaching an element of the definition, that is to say, what are called 'material points- σ '. It is not the only means by which points may be defined, at least for the physical world, as the author has shown in his paper on 'Mathematical Concepts of the Material World' (*Phil. Trans.*, 1906). Definition of the fundamental spatial concepts—points, lines, surfaces—by means of the relation of 'inclusion' T, which is a generalised form of E_σ , so that the author obtains 'material points-T', 'material lines-T', and 'material surfaces-T'. The last section of the paper is on 'material points-T' and 'material segments-T'. A paper of the greatest possible importance.] **F. Colonna d'Istria.** 'La religion d'après Cabanis.' [The letter on primary causes which was written shortly before the death of Cabanis is considered by most of his disciples as a falling away from his principles, since he explained and justified the metaphysical and religious needs of humanity. However, the author shows that there are not any essential differences between this letter and the other parts of Cabanis's work.] **L. Brunschvicg.** 'Sur les rapports de la conscience intellectuelle et de la conscience morale.' [The values of the moral conscience which are revealed to the inner man do not suffice to found "a system of things" . . . , and the values of science, abstracting as they do from quality and liberty, appear to be

incompatible with what morality spontaneously claims as an absolute. It is in these terms that it seems that the *alternative* presented itself to the thinkers of the last half of the last century. . . . We will inquire if in the actual state of our scientific knowledge and our reflexion on the sciences, the problem still presents itself to the philosopher under the same aspect ; and we will try to show how the progress of the criticism of the sciences, which has so visibly accelerated during the last twenty-five years, has insensibly re-established a kind of equality of level between our moral conscience and what might be called our *intellectual conscience*, in such a manner that the antinomy of its science and the system of morals to which previous generations have arrived has disappeared almost of itself by the sole fact of profound reflexion on scientific knowledge.] **R. Hubert.** ‘La théorie cartésienne de l’enumeration.’ [On the theory of enumeration in the fourth rule of the *Discours*. The three first rules are quite clear, their meaning is illustrated, and the *Méditations* and the *Principes* furnish new illustrations. On the other hand, it is in the earlier *Regulae* in which the theory of enumeration is exposed. ‘Intuition is a sure means, though an insufficient one, of knowledge ; and deduction, which is vitiated in its origin by the intervention of memory, is not enough to complete it. The function of enumeration is then determined : by enumerating and by making known to ourselves by comprehensive classification, by well-founded analogy, by perfect induction, all the simple elements, all the necessary relations of which nature is composed, it guarantees that a certain system of particular intuitions, that which constitutes science, reproduces exactly for us the reality of things.’] **G. Guy-Grand.** ‘Impartialité et neutralité (Méditation pour le temps de guerre).’ [‘It is important not to confuse two notions so different as *impartiality* and *neutrality*. . . . Impartiality is the supreme rule of anyone who claims to know and judge by letting his thought proceed according to strictly critical methods ; neutrality is a refusal to take part which can only end in intellectual and moral annihilation. Let us not confuse Claude Bernard, legislator of the experimental method, with Pontius Pilate, patron of “ neutrals ”.’]

‘SCIENTIA’ (RIVISTA DI SCIENZA). Series ii. Vol. xix. Part 1. January, 1916. **G. Loria.** ‘L’infinito e l’infinitesimo secondo i matematici moderni anteriori al secolo XVIII.’ [After a sketch of the emergence of science from the darkness of the middle ages, the author describes shortly and well that part of the work of Commandino, Maurolico, Stevinus, and Luca Valerio in which certain improvements of the method of exhaustion were given. Then the well-known work of Kepler (1615) and that of his defender, Anderson, are dealt with, and then a very interesting remark is made that considerations about indivisibles probably originated with Galileo independently of Kepler and owing to physical rather than mathematical questions. The works of Cavalieri, Fermat, Descartes, Pascal, Roberval, the British contemporary mathematicians, Newton, and Leibniz, are then shortly described, and a plea is made for the publication of all the manuscripts of Newton and Leibniz. Though the article does not go very deeply into the matter, it is a very able and important one.] **P. Lowell.** ‘The atmosphere of Mars.’ [At the very foundation of our modern knowledge of the body next to us in space, the planet Mars, is the problem of its atmosphere. It might seem as if the atmospheric envelope of a heavenly body were one of the least things we should care to know about it. Our own air may not casually strike us as important. Yet without it all life, animal, vegetal and even mineral, so to speak, would stop and our earth roll a

dead, immutable ball through space. Not less would Mars. Now for the last twenty years we have been steadily acquiring knowledge about the Martian atmosphere. This knowledge has come both through the telescope and spectroscope, the latter relatively lately.' An account is given of the work of V. M. Slipher and Very. 'Each step has advanced us in knowledge of the Martian atmosphere, the improvement in the instrumental means enabling us with the mind's eye to approach nearer and nearer the planet. The facts successively revealed have corroborated those which went before in an even more conclusive manner than mere repetition could. For that stands the more securely detected of which the tallying details can subsequently be distinguished.' The spectroscope has corroborated the telescope's deduction, and states what the latter has shown must be inferred: 'the presence of water and the presence of oxygen, since vegetation without either were unthinkable, and yet the penurious poverty of both'. Further, the climate of Flagstaff, where the Lowell Observatory is, has proved to be very helpful: 'Many points about the planet which seem strange to the average inhabitant of the earth become comprehensible when one's surroundings take on the setting we have learnt must exist on Mars'.] **H. De Vries.** 'L'évolution des êtres organisés, par sauts brusques.' [From the experiments of Nilsson results that amelioration is produced *discontinuously*, and not as Rimpau and Darwin believed, by a slow and almost insensibly gradual process. These discontinuities are, it is true, small, but they are of the same order as the characters which separate varieties and species both in nature and in our classifications.] **A. Graziani.** 'Le future conseguenze economiche della guerra.' [There will probably be no radical change in social constitution, but certainly there will be a redistribution of riches, and that will have considerable economic effects. The author does not believe that there will be any great transformation in the commercial policy of the different countries after the war: England will still remain free-trade, and France, Russia, Germany, and Italy will still keep to the policy of protection, tempered by agreements and treaties concluded between different countries. Economic relations will by degrees lessen the hatred which the war has generated, and 'foreign commerce will continue to develop in spite of protectionist and particularist policy, by the side of internal commerce. To wish to reduce the former and merely to increase the latter is an anachronism and an absurdity.'] **A. Weiss.** 'Le droit international d'hier et de demain.' [In future, peace must be organised and protected against the enterprises of those who wish to disturb it. International law will be the law of peace; and this is the necessary aim of an evolution which began long ago. To show this, a sketch of the history of international relations is given. The conclusion is that all progress is vain and all reform sterile if the prescriptions of international law do not obtain the obligatory force and the effective sanction which have hitherto been wanting to them. Indication of possible means whereby such sanction can be brought about.] Book Reviews. Review of Reviews. Chronicle. French translations of articles written in Italian and English.—Series ii. Vol. xix., No. 2, February, 1916. **Th. Moreux.** 'Les "Novae" et la constitution de l'univers.' [Discusses the problem of the *Novae*, the apparent stars which appear suddenly at certain periods from the depths of the celestial vault.] **F. Bottazzi.** 'Le attività fisiologiche fondamentali. Secondo Articolo: L'attività muscolare.' [The paper includes an account of the author's own physiological work and views.] **A. S. D. Maunder.** 'Iranian Migrations before History.' [From passages in two of the old sacred books of Persia—the *Vendidad* and the *Tir Yash*—the authoress deduces that they 'preserve the memory of two great pre-

historic migrations of the Iranians ; first, their migration southward from within the Arctic Circle ; and, second, their much later migration westward from the Punjab. And this continuity of memory is the more remarkable because the climate of the lands which the Iranians have occupied in historic times is in such strong contrast with the climates of which they have, nevertheless, retained so vivid an impression.' The deductions are from descriptions in the books mentioned, are of an astronomical or rather geographical nature, and are certainly ingenious. The authoress says : 'Since these descriptions could not have arisen from mere invention, or from logical deduction, they must have been derived from actual racial experience. The ancestors of the Iranians must have dwelt within the polar circle, even deep within the polar circle, and they must have found it a tolerable place of existence.]

Ch. Gide. 'Les dépenses de la guerre et leurs conséquences économiques.' [All the warring nations are spending enormous sums of money, and yet the war goes on. The explanation of this apparent paradox is simply that 'these expenses are not paid. . . . Up to the present at least these huge sums of money are phantoms ; if they become solid it will only be later on.' The author proceeds to show how this is so. It follows that no one of the warring nations has anything to fear for itself with regard to the duration of the war,—and nothing to hope for from the enemy. There is another question about which the author makes some remarks : the economic side of the war from the point of view of steel, powder, and bread. Here again the author puts things rather paradoxically. 'We may define war, speaking economically, as an industry of luxury. At the present moment it is the only luxury which the belligerents allow themselves, and it absorbs all private luxuries. I do not believe that the number of men under arms is very much greater than that of men employed in normal times in non-productive work.]

A. Loria. 'Riflessioni e previsioni a proposito della guerra.' [The probable social and economic consequences of the war.] Book Reviews. [Among them of books by E. Guyot, W. J. Ashley, and B. Ischchanian on economic and social questions in England and Russia.] Review of Reviews. French translations of articles in Italian and English.—Series ii. Vol. xix., No. 3, March, 1916.

C. G. Abbot. 'The Nature of the Sun.' [Composition and state as transmitter and receiver of energy, etc. The sun is giving off great quantities of energy without considerable return. 'Where does this radiant energy go to ? Can we imagine space to be of infinite extent and the rays continually going on to wider and wider spheres ? It seems probable that the path of the rays from the sun outwards is not infinitely great. There seems to exist in space a certain quantity of matter, perhaps partially gaseous, partially solid particles, which, though extremely sparsely distributed, still little by little absorbs the rays of light, so that in the course of a path which it may take light tens of thousands of years to travel, the intensity of a beam of light is at last reduced sensibly to zero. Apparently the final stage of things will be reached when by collisions and near approaches the mechanical energy of motion of all the stars shall have been converted into heat, and this by radiation and absorption shall be diffused until a uniform low temperature prevails throughout the universe. That this stage is almost immeasurably before us in the future is apparent. Many stars may never be rekindled by collisions, but may become dark and cold long before the final stage is reached. We have no reason to suppose that our sun will not be one of these.]

E. Bouthy. 'La théorie cinétique des gaz. Ière Partie : Ses fondements.' [In a kinetic theory we must consider, besides visible motions, hypothetical and invisible motions of molecules, atoms, and electrons.

The modern kinetic theory of gases supposes : (1) Gases and material bodies in general are discontinuous ; (2) The molecules of gases are perfectly elastic ; (3) The walls with which gases are in contact have also the property of perfect elasticity with respect to the gaseous molecules ; (4) The gaseous molecules have motions of translation distributed at random in all directions. The kinetic theory can be perfected indefinitely so as to apply to gases which show more or less deviation from the laws of Mariotte and Gay-Lussac.] **L. Matruchot.** 'Le problème du cancer éclairé par la pathologie végétale.' [The discovery of cancers in vegetables and the remarkable results to which the study of them has led have thrown a great light on the question that has for long been asked with regard to man and other animals, as to whether cancer is or is not a parasitic disease.] **O. Jespersen.** 'Réflexions d'un Danois sur la guerre.' [It would seem that these reflexions on such subjects as the feeling for nationality, militarism, freedom of the press, and civilisation, are quite just. 'From the future peace, what we must hope for may be thus summed up : *Universal and real, sacred and inviolable respect for the rights of others.* As long as we have not got as far as that, we are still living in the age of barbarism.'] **C. Supino.** 'Le fonti economiche della guerra.' [Fortunately we cannot consume future wealth. The economic problem is very complicated, since we have to study not only how we can obtain the huge sums necessary for war, but also the variations in consumption brought about by it and their effect on production. A long article.] Book Reviews. Review of Reviews. French translations of the English and Italian articles.

IX.—CORRESPONDENCE.

TO THE EDITOR OF "MIND".

SIR,—

Your reply to my note published in the last issue calls for further comment which I will make as brief as possible. It is exceedingly regrettable that those who are, presumably, interested in the advancement of philosophy should be side-tracked into the discussion of unprofitable personal issues. I am, however, obliged, in defence of my own personal honour and veracity, which certainly appear to be attacked by those parts of your original note not implicitly withdrawn, to make some additional remarks.

The question of abusive language appears to me to be worth little further discussion. Of course I do not admit that my language erred in any other direction than, possibly, undue restraint, but in the absence of positive evidence that anyone has taken serious notice of that passage I am passing on to the more important question of plagiarism. On that matter the casual reader might easily be deceived, and certainly he would carry away an entirely false impression of what I said. I had better, therefore, repeat what I actually did say, both in this journal and in the *Nineteenth Century*.

You, Sir, say that you have no concern with what was written elsewhere than in *MIND*. In that case I have said very little. My one assertion in *MIND* was that, in the pages of the "*Nineteenth Century*," I had made the suggestion of plagiarism, and that the reply, also in the pages of the *Nineteenth Century*, did not satisfy me for certain reasons. The reasons for making the original suggestion were not mentioned in your journal at all. The reader was referred back to the discussion in the *Nineteenth Century*. I could not, therefore, either make a "serious charge" or substantiate it by "specific agreements so close and detailed," etc., etc. Nor did I indicate "wide divergence between myself and Dr. Mercier". How could I indicate agreement or divergence on the relation between methodology and science when the matter under discussion was the use of universals in reasoning? Certainly I did not. If, therefore, you adhere to your statement that your comments have reference only to what appeared in *MIND*, a complete and sufficient reply is that your statements (of course accidentally and unintentionally) are entirely untrue, and that the one statement made by me in *MIND*, namely that the matter of plagiarism had been mentioned elsewhere, is true and unexceptionable.

In view of the serious misapprehensions that are bound to arise concerning what was said, and the magnification caused by this discussion it will be fairer to all concerned to give the context of and the reasons for the original suggestion. The occasion of the dispute was an article by Dr. Mercier (February, 1915) on the relation between science and logic. The article in question combined a number of uncomplimentary references to a previous article of mine with the contention that it was the business of logical theory to correct the illogical ideas of the scientific world. This contention was supported by a number of examples from current science and particularly from medicine. Let me say at once that

there is no question of Dr. Mercier copying his examples. With regard to these, Dr. Mercier's statement was, in effect, that some scientific ideas are self-contradictory. I have expressed no opinion about the value of the examples, and, concerning the basis, I think all will agree that what is self-contradictory is not true.

The reply that I did make (*Nineteenth Century*, May, 1915) was that to combine untrue statements concerning the views of any writer with an adoption without acknowledgment of the special ground and standpoint which distinguishes that writer from other logicians was reprehensible, and laid the offender open to the suggestion of a form of plagiarism. For, although I should hardly express my contention so crudely as Dr. Mercier's article appeared to me to do, the direct bearing of methodology on science has been, as I said in the last number, my own special branch of work for several years. In general terms, I do not think either you or anyone else will disagree with my reply, only you appear to contend that the standpoint is not so distinctive as I assert.

This contention I think, on further consideration, you will find not tenable. Needless to say I am not concerned to deny affinities with Descartes, Bacon, Hobbes or other philosophers of past generations. I am entirely of the opinion that any real and original philosophic thinker, if such existed at the present day, would agree with me. But the fact remains that modern logicians, at least all with whom I have come in contact, do not agree. In support of my statement it will be sufficient if I say that I have had a dispute with Dr. Bosanquet on this very point (*Journal of Philosophy*, 17th January, 1911), also that the logician chosen to make the only reply that I am aware of to Dr. Schiller's criticisms on modern logic repudiated the idea in such terms as to imply that he spoke for the great body of logicians. (*Proceedings of Aristotelian Society*, 1913-1914, p. 191, paper by A. Wolf.)

Now let us come to the conclusion of the whole matter so far as it concerns Dr. Mercier. I do not think what I have said can rightly be described as a "serious charge of plagiarism". In any case the manner in which you state it conveys a false impression. Needless to say I have no objection to Dr. Mercier finding as many examples as he pleases, whether valid or invalid, of the falsity of scientific ideas. The only matter on which it is useful to be emphatic is that, for this time and generation, the special credit (if there be any) of contending in theory and indicating in practice the intimate concrete relation between logical theory and practical science belongs to my treatment of methodology, not to the *New Logic* or to any other work or writer. This statement I doubt whether Dr. Mercier will dispute, and, unless he does so, he is not on his defence for anything more serious than an error of taste and judgment. I have not at any time accused Dr. Mercier of a deliberate attempt to gain himself credit which is not his. I have no sufficient evidence for so doing. At the same time I cannot see that anything I said, either here or in the *Nineteenth Century*, was uncalled for in view of the false impression created both by his original article and by his rejoinder to me (*Nineteenth Century*, June, 1915) that that article was a development of the ideas of the *New Logic*.¹

I am, Sir,
Yours faithfully,
H. S. SHELTON.

¹ [I am quite content to leave the disagreement between myself and Mr. Shelton to the judgment of the readers of *MIND*. I have never thought of questioning his "honour and veracity."—Ed. G. F. S.]

